THURSDAY, SEPTEMBER 5, 1895.

THE PENDULUM AND GEOLOGY.

Results of a Transcontinental Series of Gravity Measurements. By George Rockwell Putnam. *Notes on the Gravity Determinations Reported by Mr. G. R. Putnam.* By Grove Karl Gilbert. (Washington, U.S.A.: *Philosophical Society's Bulletin*, vol. xiii. pp. 31-76.)

SINCE the number of swings, which a pendulum of given length makes in a certain number of hours, depends upon the attraction of the earth at the place where it is swinging, it follows that, if an observer carries the same pendulum to different places, and notes the number of swings at each place he visits, he can by that means compare the force of gravity at the several places. If the earth were a smooth spheroid consisting of concentric shells, each of uniform density throughout, then gravity would have the same value at all stations situated on the same parallel of latitude. But if, as is the case in nature, there are mountains and elevated plateaus along the course followed by the observer, gravity ought to vary from its normal value, and in fact it is found to do so. Theoretically it is possible to calculate what variation of gravity at a given station ought to be caused by the altitude of the station, and the attraction of the neighbouring visible masses—*i.e.* of the mountain or plateau where the pendulum is swung, and of the rock masses round about, and when these disturbing causes are allowed for, and the corresponding corrections made, the value of gravity as deduced from the rate of the pendulum might be expected to tally with what it would be at the base level, supposing the mountains and all the surrounding masses carted clean away, and the smooth surface of the globe laid bare. This correction is termed reducing to the sea level, or to the mean level if the reference is made, not to the sea, but to some inland station. The question then to be answered for each station is, whether when this correction has been made, or, in technical language, when gravity has been reduced to the sea, or mean, level, does the reduction give the value which might be expected for the latitude? If it does not, this points to some deviation from regularity in the density of the earth's crust below the station, the nature of which may be inferred from the character and amount of residual discrepancy, when the reduction has been made. In this way it is that the pendulum becomes a kind of geological stethoscope.

In investigations of this kind, the elevated ground which forms the station is usually very much wider than it is high, so that, bearing in mind the law of the inverse square, it may be regarded as an extensive plain. If from local peculiarities it cannot be so regarded, compensatory allowances are made to bring it under that category. The effects of the station being situated on an elevated plateau are of three kinds, two of which cause gravity to appear smaller than it would appear at the sea level beneath the station, and one which causes it to appear greater. Of the two which make it appear smaller, the more important is, that the increased distance from the earth's centre causes the attraction of the earth as a whole to be diminished; the other, which is insignificant, and usually neglected, is that the increased distance from the axis of rotation increases the centrifugal force, which is opposed to gravity. The third effect, which causes gravity to appear greater than at the sea level, arises from the attraction of the matter of which the elevated plain, or mountain, is composed, for that may be regarded as an adventitious mass of rock, in excess of the sphere, placed beneath the pendulum. The reduction of the gravity observed at the station consists, therefore, in adding a correction equivalent to the diminution due to the elevation of the station, and subtracting a correction equivalent to the attraction of the mass of the elevated plain. If the reduction so made does not bring the observed value to agree with the value at the sea level, appropriate to the latitude of the station, there must be some geological cause present to account for the discrepancy.

It came to light in 1847, in consequence of the great trigonometrical survey of India, that, on approaching the range of the Himalayas within about sixty miles, the plumb-line, or vertical, was slightly deflected towards the mountains, so that it did not remain exactly perpendicular to the earth's surface. This was what might have been expected, because the great rocky mass would naturally draw the plumb-line towards it. But when the attraction of the mountains came to be calculated, it was discovered that, although their action was great enough to have caused a source of perplexity to the surveyors, it was nevertheless not so great as might have been expected. Clearly, then, some geological cause was latent, which required to be explained.

After some not very successful attempts at explanation by others, Airy, then Astronomer Royal, proposed in 1855 a solution of the difficulty which met the case. He assumed, as in those days was usually done, that the crust of the earth was comparatively thin, and rested upon a more or less liquid substratum, which in his paper in the *Philosophical Transactions* he called "lava." Then he showed that a great mountain mass would break the crust through unless it was supported by a protuberance beneath it, projecting downwards into a layer denser than itself. In short, it needed to be held up in hydrostatic equilibrium, much as an iceberg is supported in the ocean; and he explained how, under these circumstances, the observed deficiency of attraction of the plumb-line towards the mountains would be accounted for.

Although this observation upon the plumb-line was not a direct investigation of the force of gravity, it was nevertheless conducive to it, for the unexpected abnormality in the horizontal effect of mountain attraction rendered it probable that the same cause, whatever it might be, would produce some corresponding effect upon vertical attraction, *i.e.* upon gravity. It has been explained how the pendulum is the suitable apparatus for measuring gravity, and accordingly the pendulum was called into requisition to make more direct observations. At certain stations of the Indian Survey, of which the height and position had been already determined, the mean number of swings, called the "vibration number," was observed, which were made by the pendulum in twenty-four hours; and the force of gravity at the different stations was thus compared. The local attraction of the elevated mass on

which the pendulum stood, and the effect of elevation above the sea, were then allowed for, and the vibration number, when so corrected, was regarded as the vibration number for that station when reduced to the sea level. The pendulum used would have made 86,000 vibrations in twenty-four hours at the equator. It must therefore have been slightly longer than a seconds pendulum, which would make 86,400 in the same interval. The observations showed that there was a more or less marked deficiency of gravity over the whole continent of India, and that the deficiency was greatest at the most lofty stations. At Moré, 15,408 feet above the sea, the deficiency was enough to make the vibrations in twenty-four hours twenty-four fewer than they ought to have been if the attraction of the mountain had produced its full effect. It was obvious, therefore, that some hidden cause existed which counteracted the attraction of the mountain, and this could have been no other than a deficiency of density in the matter beneath it. The conclusion is identical with that reached by Airy in connection with the deflection of the plumb-line, namely, that the Himalayan range is supported by a downward protuberance, projecting into a more dense substratum.

This mode of support, as already remarked, is similar to what is termed hydrostatic equilibrium. As applied to the support of the earth's crust American geologists have given to it the name "isostacy," which well describes the phenomenon.

During the past year an extensive series of gravity measurements has been carried out by the Coast and Geodetic Survey of the United States, by the use of the half-second's pendulum, a much smaller and more portable instrument for the determination of gravity than any hitherto employed. Observations were made at twenty-six stations, eighteen of which follow nearly along the 39th parallel of latitude; and these are particularly well adapted to throw light on important questions regarding the condition of the earth's crust.

"This line of stations, commencing at the Atlantic coast, ascends to near the Appalachians, traverses the great central plain, gradually increasing in altitude from 495 to 6041 feet, then rises to the high elevation of the main chain of the Rocky Mountains, reaching an altitude of 14,085 feet at Pike's Peak, descends into the eroded valleys of the Grand and Green Rivers, crosses the summit of the Wasatch ridge, and finally descends to the great western plateau of the continent."

This series of gravity determinations affords an exceptionally favourable opportunity of helping to determine whether the support of the elevated regions traversed appears to be best accounted for by rigidity in the foundations on which they rest, so that, in spite of their weight and the largeness of the area occupied by them, they are prevented from sinking down into the material beneath; or, on the other hand, whether they are supported, as we have said that Airy suggested, namely by floating in a denser substratum, or, as the Americans say, by "isostacy," which is the same thing as hydrostatic equilibrium.

The general principle of the method pursued in reducing gravity to the sea level has been already explained. It consists in adding a correction equivalent to the diminution of gravity due to the elevation of the station, and subtracting a correction equivalent to the attraction of the mass of the elevated plain upon which the station may be considered to be situated. When these two corrections have been made, gravity so corrected would be the same as that appropriate to the latitude, or, as it may be termed, to the "computed value," unless there is some deviation from regularity in the density of the matter below sea level. The result proved that this was the case. For gravity so reduced turned out to be invariably less than that appropriate to the latitude. It was clear, therefore, that at these stations in America there was a deficiency in density beneath the elevated districts, just as had already been found to be the case in India. There could be no doubt that isostacy had a share in contributing to their support. The inquiry was now carried a step further. Did each mountain individually owe its support to a separate protuberance of its own beneath it, or was the mountainous region as a whole supported in that manner, each separate mountain owing its support to the strength of the crust on which it was a mere excrescence? The case might be illustrated by conceiving a number of logs of wood of different sizes. If these float side by side in water, the larger logs will stand the higher above the surface of the water; but each log will have a part immersed which will be its individual support, and this will be deeper for the logs which stand the higher. But if these logs are placed upon a raft, the support will be general, and derived from the support of the part immersed of the entire raft, and its depth will depend upon the aggregate weight of the logs. Nevertheless it need not dip deepest beneath the logs which stand the highest above the water, or above the floor of the raft.

The presumption was against each elevation being separately isostatically supported, because the deficiency in gravity, and therefore in density, was not found to be greatest precisely beneath the highest stations. To carry out the inquiry more fully, it was considered that, by omitting the part of the reduction to the sea level which takes account of the attraction of the mass of the plain (which would mean omitting to subtract the attraction produced by it), we should, as it were, transfer its mass to the subjacent parts, and so make up for the lack of density, and obtain the condition of uniform density below the sea level. There would then remain only the correction for elevation necessary. If this proceeding gave the value appropriate to the latitude under each station, it would show that the individual stations were seriatim in isostatic equilibrium. But the attempt failed. It was found that the attraction of the matter of the more elevated stations was not separately compensated by defect of density immediately below. The analogy of the detached floating logs did not hold good. It remained to inquire whether the series of stations was in isostatic equilibrium when considered as a whole—the case more nearly analogous to the raft. If this were so, gravity, when reduced to the sea level, would be uniform for the whole tract.

For this purpose a mode of reduction devised by M. Faye was adopted. The altitude of the country surrounding the station within a radius of 100 miles was reduced to a mean altitude, and the attraction of a plate of rock of thickness equal to the difference of altitude between this mean

plain and the station was allowed for, and it was found that this correction brought the gravity at each station much nearer to the computed value for the latitude than either of the previous methods. The conclusion was that, when large areas were considered, they were approximately in isostatic equilibrium. "The result of this series [of observations] would therefore seem to lead to the conclusion, that general continental elevations are compensated by a deficiency of density in the matter below sea level, but that local topographical irregularities, whether elevations or depressions, are not compensated for, but are maintained [supported] by the partial rigidity of the earth's crust." (Putnam.) "The measurements of gravity appear far more harmonious when the method of reduction postulates isostacy, than when it postulates high rigidity. Nearly all the local peculiarities of gravity admit of simple and rational explanation on the theory that the continent as a whole is approximately isostatic, and that the interior plain is almost perfectly isostatic." (Gilbert.)

It appears therefore that the crust of the earth is sufficiently thick and strong to carry such unequal loads as considerable mountains upon its surface without necessarily breaking through; but, when a large area is involved, it bends downwards into a denser material beneath, so that the crust and the load it carries are conjointly in approximate hydrostatic equilibrium.

O. FISHER.

SOME RECENT BOOKS ON MYCOLOGY.

British Fungus-Flora. A Classified Text-book of Mycology. By George Massee. Vol. iv. 8vo, pp. viii. 522. (London and New York: George Bell and Sons, 1895.)

Systematic Arrangement of Australian Fungi, together with Host-Index and List of Works on the Subject. By Dr. McAlpine, Government Vegetable Pathologist. 4to, pp. vi. 236. (Melbourne: Robt. S. Brain, Government Printer, 1895.)

Guides to Growers. No. 18. *Onion Disease.* By D. McAlpine. (Victoria: issued by the Department of Agriculture, 1895.)

MR. MASSEE is to be congratulated on the completion of another volume of his "British Fungus-Flora." There has been no complete work of the kind issued since the publication of M. C. Cooke's "Handbook of British Fungi" in 1871, and the knowledge of these obscure plants has advanced enormously since then. In the first three volumes the author treated the *Basidiomycetes* and the *Hyphomycetes*; the present volume takes up the large natural order of the *Ascomycetes*, and deals in turn with three families—the *Gymnoascaceæ*, the *Hysteriaceæ*, and the *Discomycetes*. The *Hysteriaceæ* form such a natural transition between the *Discomycetes* and the *Pyrenomycetes*, that it seems a pity Mr. Massee has not so arranged the families as to make them follow each other in the text-book; he has, however, very carefully pointed out the affinities of the different groups.

A general account of the *Ascomycetes*, their life-history, habitat, &c., is given in the introduction. The author agrees with Brefeld that sexual reproduction is unknown in this family. There is also some useful information about the best methods of collecting and preserving specimens, and of examining them. New descriptions have been written out for many of the plants, based in nearly every case on the author's own observations. Wherever it has been possible, he has examined the type specimens, or those specimens accepted as authentic in well-known *exsiccati*. It is impossible to over-estimate the value of such work. The descriptions are full and complete, and great care has been taken to give careful measurements.

The *Hysteriaceæ* have not before been worked up for Britain. Mr. Massee has not included *Acrospermum* in this family, nor in this volume. We await the next instalment of his work, to see where he will place it.

"British Discomycetes," by Mr. W. Phillips, has been for some time the standard work for that family. It was published in 1887, and there has been no reason for any material change in the way of treating the subject. The genera *Xylographa*, *Biatorella*, and *Abrothallus*, previously included among lichens, have been proved to be fungi, and are recorded, *Xylographa* in the family of the *Phacideæ*, *Biatorella* and *Abrothallus* in the *Patelliariæ*.

The classification of the fungi is pretty well fixed as regards the natural orders, but no two systematists are agreed on the arrangement of genera and species. What characters are important enough to constitute a genus, is a question that each one answers in his own way. Phillips gave great importance to microscopic characters, but he was also largely guided by features visible to the naked eye or on slight magnification. He has comparatively few well-marked groups, and somewhat large genera with sub-genera. Saccardo laid much more stress on the differences between the species, and created new genera to represent deviations from the types, or revived old genera that had been sunk by systematists like Phillips. Mr. Massee goes even further; he retains nearly all the genera that had been kept up by Saccardo, and he has added in the *Discomycetes* eight genera revived from older authors, and five new genera, none of these being founded on new plants. Mr. Massee may be right in his views of classification, but the multiplication of genera and species, where that can be avoided, is much to be regretted. The matter has been admirably stated by Mr. Spruce in his "Hepaticæ of the Andes and Amazon," p. 73. "For a local flora," he writes, "or a limited area, too many genera will tend to produce confusion rather than precision, especially where several of the genera are monotypic; so that, on the whole, it seems desirable to make our genera as comprehensive as possible." There are several monotypic genera included in this volume, as for instance *Cubonia*, to which genus *Ascophanus Boudieri* has been transferred on account of its globose spores, those of *Ascophanus* being elliptical.

The task of classifying the *Pezizæ* is no light one; they are here divided into three large groups—*Glabratæ*, *Vestitæ*, and *Carnosæ*, under which the genera and species are arranged in a way that differs, in many instances, from that of every previous writer. The two first groups are familiar to us as the *Nudæ* and *Vestitæ* of Phillips. In the latter group the genus *Lachnella* has been dropped, and the species are dispersed and reclassified under *Erinella*, *Echinella*, *Diplocarpa*, *Dasyscypha*, &c. *Lachnella Cupressi* has been placed by itself in the genus *Pitya*, because the margin is free from the external hairs that are so marked a feature of this group, and because it

grows on conifers! In this group we also find *Geopyxis* Persoon (emended) Myc. Eur. i. p. 224 (not p. 42, according to both Saccardo and Mr. Massee): Persoon did not make *Geopyxis* a genus, although Saccardo also credits him with having done so; he published it as a division of *Peziza*, and Saccardo is the first who made it a genus, and therefore it ought to be *Geopyxis* Sacc. One of the species is the beautiful *Peziza coccinea* of old authors, transferred by Phillips to *Lachnea*, by Saccardo to *Sarcoscypha*, and now by Mr. Massee to *Geopyxis*. The division of the *Carnosæ* includes the genera *Peziza*, *Otidea*, *Humaria*, and others. A new genus, *Curreyella*, has been made to include *Peziza radula* and *P. trachycarpa*. Are we to assume that the Cuban species *Massea quisquiliarum* grows also in Britain?

In the family of the *Helvelleæ* there is much less alteration and rearrangement; but even there, two genera have been retained that were considered unnecessary by Phillips and Saccardo: *Cudonia* Fr., to contain *Leotia circinans*, which differs from others of the genus in the possession of filiform spores, and *Mitrophora* Lév., in which are placed two species of *Morchella*, *M. gigas* and *M. semilibera*. In these the lower half of the pileus is free from the stalk.

The numerous changes, however much we regret them, testify to the care with which Mr. Massee has treated the subject. He has omitted to mention one point of considerable morphological interest: that the abnormal many-spored condition of the ascus in *Tympanis* is due to budding of the original eight spores in the ascus.

The classified list of fungi, issued by Dr. McAlpine, has been compiled to assist vegetable pathologists in determining the diseases of plants due to these organisms. The knowledge of Australian fungi is as yet very incomplete, and we may expect the list to be largely augmented. M. C. Cooke's "Handbook of Australian Fungi" has served as a basis for the present work, and to it have been added the genera and species recorded by the more recent collectors and workers in this branch of botany. Australia possesses such a unique flora of the phanerogams, that we should have liked some indication of the fungi that belong exclusively to that country. The author has mainly followed the method of classification which has been adopted by Saccardo in his "Sylloge Fungorum." Dr. McAlpine retains the *Hyphomycetes* as a class by themselves, but describes them as imperfect *Ascomycetes;* this is hardly correct, for though many of them have been proved to be form-genera, others are unrelated so far as is yet known.

Besides giving us a list of fungi, Dr. McAlpine has drawn up some very instructive tables. The number of fungi recorded varies very much from colony to colony. Victoria heads the list with 1070 species, though we suspect this position of pre-eminence is due to the presence of Baron von Mueller, rather than to the abundance of fungi. Queensland records 1060 species, a large percentage of the whole due to the labours of an indefatigable worker, Mr. F. M. Bailey. Brisbane has 739 species, and New South Wales lags far behind with 406. There is much work evidently to be done before the localities are all worked out. The total number for Australia and Tasmania is 2294, as compared with 5040 recorded for Britain. The total number of species known to science is somewhere about 40,000. Dr. McAlpine has also prepared a host-index, which presents many points of interest. On *Casuarina*, that peculiar Australian tree, we find *Fomes igniarius*, a cosmopolitan species. *Eucalyptus* seems specially afflicted—leaves, bark, branches and trunk have all their separate fungal parasites. The *Compositæ* are hosts to but two, an *Æcidium* and a *Synchytrium*, evidently an incomplete account.

The *Agaricineæ* and *Polyporeæ* have received a much larger share of attention than the more minute forms of the *Discomycetes* and the *Pyrenomycetes;* Australian collectors give an account of but five *Nectrias* and two *Valsas*, but these forms are very easily overlooked. The *Phycomycetes* are also very sparingly represented; there are two *Peronosporas*, one on tobacco-leaves, the other on the onion. There is no record of potato disease, nor of salmon disease; we can only congratulate the colony on its immunity.

In addition to the authority and date for each fungus, Dr. McAlpine gives the locality in Australia, the habitat and a description in English of the species, but in no case does he indicate the characters of the genus; the list thus strikes the reader as being very imperfect, and the absence of all information as to the size of the particular plants renders it less useful than it might otherwise have been. We think he has vainly spent his strength in his attempt to provide an English equivalent for the scientific name of each fungus. Popular specific names have not been given even to flowering plants, such as the different kinds of *Myosotis* or *Crepis*, and such names are equally valueless in the case of fungi.

Dr. McAlpine has recently published, in "Guides to Growers," a most useful and practical account of the disease of onions caused by eelworms, with the best methods of cure. The worms live in the soil, and various dressings are recommended, suitable rotation of crops, or burning the surface of the land. This particular eelworm attacks the stems of plants, and in the case of the onion destroys the bulbs, leaving the roots unharmed.

A. L. S.

OUR BOOK SHELF.

The Climates of the Geological Past, and their Relation to the Evolution of the Sun. By Eug. Dubois. (London: Swan Sonnenschein and Co., 1895.)

THE first part of this essay consists of a brief and judicious summary of the geological evidence as to great changes of climate in past ages, while the second part is an attempt to explain the causes of such variations. Various well-known theories have been advanced to account for the phenomena, but none have met with general acceptance; a few years ago Dr. Neumayr wrote: "Most plausible and simple would it certainly be were the sun a variable star that at different periods emits different quantities of heat; but for this no proof is forthcoming." (NATURE, vol. xlii. p. 180.) The author of the present work seems to have adopted Dr. Neumayr's suggestion, but goes further and attempts to show that the postulated changes of solar radiation have actually taken place. In a general way, the fact that the sun must once have been hotter, has been frequently stated as a possible cause of the higher temperatures during early geological times, but a gradual cooling of the sun is insufficient to explain all the vicissitudes of geological climates. Basing his estimate on the relative proportions of stars of different spectroscopic types, the author considers that the sun has

passed about three-fifths of its star life, and that we cannot be far wrong in assuming for the past a maximum duration of about ten million years, and a radiation in the white-star stage twice as intense as at present. As a step towards the reconciliation of the life assigned to the sun by physicists and that demanded by geologists, it is suggested that in consequence of the higher temperature when the sun was a white star, denudation was carried on more vigorously, and animal and vegetable life developed more rapidly than has been supposed.

Notwithstanding that the author has approached the subject with an enlightened mind, he does not appear to have greatly advanced the explanation. For the production of changes other than those due to the progressive cooling of the sun, it is necessary to suppose that the sun is subject to periodical changes, and the chief argument brought forward in favour of this supposition is that the acknowledged eleven-yearly period of the sun renders it probable that there may also be periods of longer duration.

It is clear that such long-period changes are quite outside our range of observation, and the indirect evidence brought forward is unconvincing. We do know, however, that the variation which has been observed in stars resembling the sun is very rare and always slight.

Methodisches Lehrbuch der Elementar-Mathematik. Von Dr. Gustav Holzmüller. (Leipzig: Teubner, 1894–5.)

THIS is a text-book of elementary mathematics, showing the extent of knowledge required of the German schoolboy; and apart from the interesting presentation of the subjects in a manner far superior to anything we can show, the book is well worthy of translation as illustrating the difference in the standards of requirement of German and English schools; the knowledge exacted of the German schoolboy being about the equivalent of our B.A. requirements.

But then the German schoolmaster, although working to a much higher standard, can take his responsibilities lightly; he has merely to point out to his pupils that it depends entirely upon themselves whether they are to spend three years or only one under the civilising influence of the drill-sergeant.

The harder his pupils work, to escape with one year of military service, the higher the standard which the government inspector can exact for exemption; thus the paradoxical result is attained that the system of conscription tends ultimately to elevate the intellectual standard of school knowledge; but, on the other hand, the physical development of youth runs great risk of being stunted. Obviously there is no place in a German school, or French school either now, for the cricket, rowing, and football, which we now consider of equal importance with abstract studies. All Europe is now an armed camp, this country excepted; and the observant philosopher is doubtless beginning to draw inferences as to the comparative effect of the systems on the development of the human race.

Dr. Holzmüller's "Einführung in die Theorie der isozonalen Verwandschaften und der Conformen Abbildungen," 1882, is a well-known standard work, profusely illustrated with carefully-drawn diagrams, which emphasise many delicate points in the Theory of Functions in a manner much more convincing than arguments depending on a procession of analytical formulas; so also in this "Methodisches Lehrbuch," a plentiful supply of figures serves as a substitute for long algebraical calculations.

The author has made these elementary parts of mathematics more interesting and pleasant reading by historical notes and simple applications; and altogether the work is a great contrast to the dry bones we are accustomed to here; it would be well for our writers of school books to study the sentiment expressed in Dr. Holzmüller's preface: "Uns von der allzustarren Gebundenheit der Lehrpläne zu befreien." G.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Heights of August Meteors.

IN addition to the four or five meteors recorded last week in NATURE (vol. lii. p. 395–6), by Mr. Denning, as having been simultaneously observed at more than one station during this year's August-meteor period, particulars which have just now reached me of some observations of the Perseids made at Tring, in Herts, on the night of the 11th inst., show that two of the meteors seen and mapped here between 9.45 and 12 P.M. on that night, also had their apparent paths mapped simultaneously at a place at Tring, about nineteen miles due north from this point of observation. The base-line between the two stations is a rather short one for such comparative determinations, but as the recorded flights passed nearly overhead, and nearly from east to west across the line between the stations, the conditions for accuracy were very favourable in both the meteors' cases, and the apparent paths seem also, by the comparison, to have been mapped with much precision. They require, however, as usual, corrections of a few degrees at the beginning and end points to make them quite geometrically compatible.

Tring.—August 11, 9.53 P.M.; 1st magn.; left a long streak along a long path; 33°, from 332½ + 39 to 287½ + 42½; (corrected path, 35°, from 328 + 40 to 280 + 41). Duration, 2 or 3 seconds.

Slough.—August 11, 9.53 P.M.; 1st magn., white; 37° in 1·2 second, from 331 + 53 to 268 + 51; (corrected path, 35°, from 336 + 50½ to 277 + 54). Left a bright white streak on its whole course for 3 seconds.

The observed tracks are 15° to 13° apart, nearly parallel, but slightly converging; and if made parallel, about 14° apart throughout, they indicate a radiant-point at the east-horizon (11° N. from E.), at 21 + 7, near μ and o Piscium, from very near which radiant-point the meteor was, no doubt, directed, as its long streak-leaving flight plainly enough denoted a very nearly horizontal motion. The resulting real path is from 77 miles over a point 4 miles north of Farringdon, in Oxfordshire, to 77 miles over a spot 3 miles E.N.E. from Uxbridge, in Middlesex. This course of 50 miles, with a duration of 1·2 seconds, gives the speed of flight 41⅔ miles per second, the speed for meteors with parabolic motion from the same radiant-point (omitting a small addition for the earth's attraction) being 40¾ miles per second. In Mr. Greg's "General Comparative Table of Radiant Positions," as No. 106 of the list ("British Association Report," 1874, p. 333), a place at 22 + 5 is given as the average radiant-centre of a group of several meteor showers observed by Schmidt, in Athens (p. 321–2 of the same "Report"), in July, August, and September. It was thus from a very central direction of a rather notable autumnal group of meteor-showers in the neighbourhood of α Piscium, that this bright streak-leaving meteor seems to have proceeded. The corrections above applied to the recorded paths, although apparently considerable, are really only slight shiftings of the flights lengthwise; their original lines of direction, and hence their resulting radiant-point being left, as nearly equally as possible in both the paths, almost unaltered.

Tring.—11.3 P.M.; shot 12°, leaving a streak, from 345 + 58 to 325 + 53 (corrected path, 13°, from 343 + 58 to 322 + 52).

Slough.—11.4 P.M.; 3rd magn.; shot 10°, without streak, from 350 + 72 to 312 + 70 (corrected path, 10°, from 352 + 72 to 317 + 70½).

The path corrections here are only small shortenings or extensions of the apparent tracks to bring their lengths into agreement (at distances apart then of 13° to 18°, in the right directions) without disturbing the path-directions, which diverge from 45 + 53, a point nearly coinciding with the usual radiant-point of the Perseids on August 10–11, at 44 + 56.

The concluded real path is from 67 miles over a point 5 miles west of Leighton, in Bedfordshire, to 53 miles over a point 3 miles west by north from Tring. The length and downward slope of the real path was 19½ miles, from 45° altitude, 34° north from east. The time of flight of this small Perseid was not noted at either place of observation, but as it probably agreed with that of several similar short Perseids noted nearly overhead on the

same night, which varied with the lengths of path from about 0·5 to 0·7 sec., the velocity deduced would be between 39 and 27 miles per second, fairly agreeing with the real meteor-speed of the Perseids, which is 38 miles per second.

It would be interesting to learn if any observations were made elsewhere of a meteor seen at Tring at 9.32 P.M. on the 19th inst., descending from near the zenith at $317\frac{1}{2}+37$, southwards to $320+15$, which exceeded the fixed stars in brightness, and which was brilliant enough to attract the attentions of ordinary wayfarers there; so that with this observation of its path at Tring, its real course, and the position of its radiant-point in the northern sky might be determined. A. S. HERSCHEL.

Observatory House, Slough, Bucks, August 29.

Do the Components of the Compound Colours in Nature follow a Law of Multiple Proportion?

ON examining the data contained in Mr. Pillsbury's interesting and valuable *résumé* on colour measurements in the United States, by means of ordinates and abscissæ for the various colours on squared paper, it became at once evident from the parallelism of the diagonals which could be drawn that, although previously hidden, there was a numerical relation underlying them, and that probably the measurements would furnish an affirmative answer to the question printed at the head of this letter. Can it possibly be that those compound colours which occur with such profusion in nature are the result of simple colours being combined in definite multiple proportions? Can there be a law of multiple proportions here, similar to that which holds good in the domain of chemistry?

Let us see how far the data which Mr. Pillsbury gives support such a conclusion: they cannot from their paucity prove it. If we take all the foliage greens given, raise the percentage of black to 100 in each case, and proportionately increase or decrease the yellows and greens, then the amount of yellow in each case divided by the amount of green in each case will give a ratio which, the black being equal, may be said to represent in figures the colour of the particular foliage. Now what do we find on examining the resulting ratios? They are all divisible into groups of multiples of 2, which may be represented as in the last column of the table by 1-Y, 2-Y, 3-Y. It will be noticed that while the figure in the second decimal place is not exactly a multiply of 2, yet it tends very much in that direction.

	Black.	Yellow.	Green.	Yellow ÷ Green.	Yellow.
Hemlock-Spruce	100	2·25	10·1	·22	1-Y
White Pine ...	,,	2·9	12·8	·22	
Apple	,,	6·25	3·75	·48	
Hornbeam ...	,,	6·8	15·3	·45	2-Y
Hickory ...	,,	5·3	11·1	·47	
White Birch ...	,,	6·8	14·1	·48	
White Oak ...	,,	9·3	14·3	·65	3-Y

Of course the conclusion reached cannot by any means be considered proved, as we do not know if the foliage greens were selected by Mr. Pillsbury purposely, or were merely the result of pure chance; but it would seem amply to repay further investigation, and I should be pleased to hear that Mr. Pillsbury could undertake it, or, if he feels unable, but would furnish me with the necessary material, I would try and undertake it myself.

As much stress is laid upon the commercial utility of this system of colour measurement, might I suggest that in all cases the simple colour of which there was the largest quantity should be taken as measuring 100? By this means there would always be one less number to recollect, write, or telegraph, than there are simple colours in the compound—no small factor when dealing with large quantities. F. HOWARD COLLINS.

Churchfield, Edgbaston.

Transformation of Moulds into Yeasts.

THE experiments carried out in Dr. Jörgensen's laboratory in Copenhagen, on the morphological relations of moulds and yeasts, are of great interest, and have an important bearing upon the study of the Japanese method of *saké*-brewing, an investigation of which was made by the writer whilst in Japan, and the results of which were published by the National University of Japan in 1881.

In this process a mould is caused to grow over the surface of steamed-rice until the grains are firmly matted together by the fibres of the mycelium, and this product (*koji*), mixed with fresh steamed-rice and water, is placed in mash-tuns and slightly warmed. After a short time active fermentation sets in, resulting in the preparation of a liquid (*saké*) containing as much as 15 per cent. of alcohol. The question as to the origin of the ferment-cells was discussed in the memoir above referred to, and the conclusion was arrived at that there was no evidence to show that the mycelium underwent any change, but that the ferment-cells were derived either from the air, or from the vats, or from spores which had attached themselves to the surface of the mycelium. Microscopic drawings were given illustrating the appearance of the mash at various periods during the fermentation, and in these the mycelium was seen to preserve its original form to the end of the process. The feature upon which most stress was laid by those who asserted that the mould was converted into the ferment, was the suddenness with which the fermentation showed itself, of which fact there could be no doubt; but it seemed to the writer that there was a very simple explanation of this. The fermentation appears immediately after the warming of the mash, which has already been exposed to the air in shallow vessels at a low temperature for several days before being collected into a single vat. It is also allowed to stand in this vat for several hours before heating, during which time probably all the dissolved oxygen has been used up by the ferment. By heating, the temperature is then raised to about 25 C., a condition very favourable to the growth of yeast. Knowing how rapidly the yeast plant buds under such conditions, it does not seem necessary to invoke the transformation of the mycelium into ferment-cells to account for the sudden appearance of the fermentation.

The note (NATURE, August 22, p. 397) further says that Juhler found in his flasks in which the Japanese mould, *Aspergillus oryzæ* (called *Eurotium oryzæ* in the writer's memoir), was cultivated a growth of typical alcohol-producing saccharomyces cells. If there were spores attached to the surface of the mycelium, it seems necessary to know in what manner they were destroyed before the introduction of the mould into the culture flasks. It would also be interesting to have more details of the size of these cells, to ascertain if they correspond exactly with those found in the native Japanese fermenting vats. The size of the full-grown cells measured by the writer were on the average 0·0082 m.m. in their longest diameter—that is, between the dimensions of ordinary beer-yeast and wine-yeast.

Cardiff, August 24. R. W. ATKINSON.

IN reply to Mr. Atkinson's inquiries, we would refer him to Juhler's original communication on his experiments with *Aspergillus oryzæ*, to be found in part ii. of the *Centralblatt für Bakteriologie*, Nos. 1 and 9, 1895.

August 29. THE WRITER OF THE NOTE.

Mr. Seebohm on Middendorff's Credibility.

MR. SEEBOHM writes (*antea* p. 385): "There is no reason to believe that Middendorff ever found the eggs of the little stint. The eggs which he records as being those of *Tringa minuta* were probably those of *Tringa ruficollis* and possibly those of *Tringa subminuta*." To me these statements seem made in oblivion of the facts, and as some years ago I exhibited in London (*Proc. Zool. Soc.*, 1861, p. 398) one of the specimens on which doubt is thus thrown, I beg leave to show that there is no reason for believing that distinguished explorer to have been mistaken. The only eggs he records (*Sib. Reise*, II. ii. p. 221) are four, the parent of which he caught under his game bag. No other nest is mentioned, and this one was found on July 1, 1843, in the Taimyr peninsula, which is admittedly as well within the range of *T. minuta*, as it is outside of that recorded for *T. ruficollis* (*cf.* Palmén, *Vega-Exped. Vetenskapl. Jakttagelser*, v. tab. 4). Though not recognising these two birds as good species, Von. Middendorff had carefully noticed (*tom. cit.* p. 222) the difference between examples obtained in the far East (Ochotsk) and in the high North (Taimyr), expressly stating that the latter agreed with Naumann's figure which undoubtedly represents *T. minuta* in summer plumage. As to *T. subminuta*, I am not aware of any evidence of its occurrence in the Taimyr, and by conjecture only can it be ascribed to that district; but the supposition that a single nest can have belonged to both *T. ruficollis* "and possibly" *T. subminuta*, is a masterpiece of conjecture wholly above my power of comprehension.

ALFRED NEWTON.

Magdalene College, Cambridge, August 23.

ON PHOTOGRAPHS OF THE MOON TAKEN AT THE PARIS OBSERVATORY.

QUITE recently some negatives of photographs of the Moon, taken at the Paris Observatory by MM. Lœwy and P. Puiseux, were exhibited at the Academy of Sciences.

The negatives have been carefully studied, enlargements made, and specimens sent to all the principal scientific societies interested in them. These enlarged copies are of great help in the study of the Moon, and have been the means of making clearer many uncertain points, for they allow every detail to be seen without difficulty. Their chief advantage, however, lies in the great expanse of surface which they embrace; many facts, hard to discover on the smaller negatives, have now been ascertained.

In their communication made to the Academy, MM. Lœwy and Puiseux gave an account of the results they have obtained in studying these photographs. Some of them are of great interest.

Considering, first, the Moon's surface, they note that its markings are of a less varied type than those of the earth, and its prominences are chiefly of a circular shape. By the way in which the Moon reflects, it is thought that its crust is of solid matter, similar to volcanic rocks. This agrees perfectly with Laplace's hypothesis, in which he states that the Moon was thrown off from the earth when the latter was in a nebulous state. The Moon's mean density scarcely surpasses that of the crust of the earth; its materials, judging exclusively from the exterior crust, are of a more uniform chemical composition. But although we might trace its history from the time in which it was thrown off from the earth, it is clear that all the facts rest on a very uncertain basis; it is scarcely probable that the Moon had the same appearance then it has now; it is only when the masses had become to a certain extent solid, that the surface-markings could have been formed which are now to be seen. A very long period must have elapsed between the nebulous state of the Moon and its present fixed condition, the process commencing, no doubt, by the union of the particles of scoriæ. Owing, however, to currents arising from various sources, ruptures must often have taken place, causing lines to be left on the parts which were not quite solid.

The various lines, which can be followed on the photographs, may be quite easily described. They are valleys between huge mountains. One of the largest is the valley of the Alps, to the west of Plato; another one between Herschel and Hipparchus, between Bode and Ukert; and one to the south-west of Rheita. It would be absurd to imagine them anything like the terrestrial valleys; they are almost perfectly straight, do not branch off at all, and keep the same width almost the whole length. There is no sign of what has become of the materials out of them, and when minutely examined, they appear to have flat bottoms; this fact seems to prove that they were once filled with some liquid which has dried up. As before stated, their origin is most probably due to currents, which must necessarily have developed in the mass of the moon when still fluid. These valleys are grouped about in various parts, and run parallel chiefly, especially near the equator, but they also go in other directions. There is nothing to show that the direction has remained the same.

So long as the revolution and rotation of the Moon were not performed in the same time, the tides must have produced very considerable change of level, which would hinder the crust from becoming solid. The scoria, therefore, would gradually form itself into larger and larger islands, which, however, might often have got broken up owing to constant collisions. Still gradually gaining in thickness, they eventually constituted the oldest part of the Moon, and at their expense the circular formations were formed which we now see. After a time banks of scoriæ of great length covered the Moon, leaving only narrow passages for circulation. Continual collisions destroyed the projecting parts, which facilitated the ultimate joining of the islands.

The fluid masses of a body like the Moon take part in the general circulation, but naturally have their tides under the influence of gravity. The combination of these two movements produces irregular rates in the floating masses, which more or less always impede their displacement. This irregular rate causes renewed collisions and rectilinear formations differing in direction from the first. After such various forces had been brought into play, it is not astonishing that the marks left are not absolutely regular and symmetrical. The parallel lines indicate the existence of similarly directed currents at the time the superficial solidification was going on. The lines running in different directions, indicate changes in the direction of those currents.

Let us now consider the result of a huge boulder of crust getting detached and falling. If falling on a slope, it would naturally slip down, and in the matter, not yet solid, form, as it were, a path; thus ultimately a valley would be made. This explanation applies itself more especially to the valley of the Alps, because of its very precise shape. If, therefore, these valleys are imperfect joinings of ancient ruptures, they must form, on the hard crust, lines of less resistance. The lines of craters are now easily explained, also the various holes in the furrows, which may be looked upon as explosion outlets.

If, on the other hand, they date from superficial solidification, their presence must have influenced the subsequent formations. Admitting that, under a part of the crust already thick, a diminution of pressure is produced, capable of producing a cavity, these changes might be brought about by the gradual cooling of the Moon, or by the movements of the interior tides. The cavities might take almost a circular form if the crust were homogeneous, having for the centre the point where the pressure was at a minimum. But if there are other ruptures and lines, they would probably form the boundary to the cavity. We notice that the polygon form is most frequent after the circular; in many cases, also, the furrows form tangents to the circles.

MM. Lœwy and Puiseux remark, finally, that it is not for them to say which of the hypotheses is correct; they merely wish to call attention to the immense help the enlarged negatives may prove themselves to be. Eventually, no doubt, they will be the means of making a map, which may show us that the surface structure of the Moon is very similar to our own.

We imagine that not every one will agree with all the opinions above expressed by MM. Lœwy and Puiseux, but it is clear that several important questions have been raised by the magnificent photographs we owe to their skill and industry.

UNSCIENTIFIC EXCAVATIONS IN EGYPT.

PROF. DR. G. SCHWEINFURTH has recently written a most interesting letter to the editor of *Die Zeitschrift für Aegyptische Sprache*. According to him, the time has arrived when a limit ought to be put to the energy of Egyptian excavators. Within the last few years there has been such a tremendous collecting of antiquities, that it has seemed to be the desire to leave nothing whatever for the next generation to discover. Our near descendants will, in all probability, not thank us for our want of patience; it may have totally different methods of research, and may bring opinions forward we have not dreamt of. If this be the case we shall, most probably, be blamed for having dis-

turbed so much, and shall be accused of "vandalism" under the mask of advancing scientific research.

There is no doubt that the excavations have been carried on too fast. The great museum in Egypt has no proper catalogue, and is arranged and filled up with things in a most unsatisfactory way; many objects have not even got the date when found. In this way, what would be treasures have become absolutely valueless on account of the carelessness of former officials, who constantly depended on each other, and, in many cases, on their memory, for the facts connected with objects found. This will always be the state of things unless the excavations are supervised by museums; for the haste with which they are carried on, does not allow time to work out the history properly. The things are merely brought under cover; they accumulate, and only short notices are written about them. It is for this reason that many noted things found have not been heard of till years afterwards; likewise, before the old treasures were properly examined, others have been dug out, particular attention being given to pretty things with which to ornament museums. Consequently, while search was being made for inscriptions, smaller objects were neglected, and many details overlooked.

Whereas formerly complaints were ever being made about the difficulty of obtaining permission to excavate, now the state of things is just the opposite. There is too much liberality; men are allowed to excavate, who have no knowledge whatever as to how to set about it, and have no serious object in view. Valuable things have been removed from the Fayûm, Heliopolis, and other places by quite uneducated people, and sold as market goods in Cairo. All this sort of thing makes the advancement of science a farce.

A natural consequence of this hasty digging, but a state of things greatly to be lamented, is the destruction of the ancient topography. The confusion caused is beyond description. It is very desirable that there should be an international inspection committee, which would insist on things being cleared up, and not allow the graves and tombs to remain open, with bones and limbs of the dead in them, which is so often the case.

Another deplorable fact is the absolute ignoring of objects connected with natural history. These objects require special care when being dug out, and also are more difficult to find. Their destruction greatly endangers the science of antiquity, and many an object, the value of which is now unknown, may in some future period be the key to some great problem. Likewise the bones of domestic animals are overlooked, although the many pictures of these animals help to make a study of them very interesting, and the remains of plants and flowers are similarly neglected, though these objects are the stepping-stones to the restoration of the ancient history.

JOSEPH THOMSON.

By the death of Mr. Joseph Thomson, we have to mourn one of the foremost of contemporary African explorers. His loss is all the more sad, as it comes in what ought to have been the prime of his manhood. When we remember what Thomson has done, what a part he played in the exploration of British East Africa and in securing for England her supremacy on the Niger, it is difficult to realise that he has done it all before the age of thirty-eight. Joseph Thomson was born in Dumfriesshire, on February 14, 1858, and was the son of a quarry-owner. He was educated at Edinburgh, and early took a keen interest in African exploration, in which he first personally joined as assistant in Keith Johnston's expedition to the African Lakes. This left Dar-es-Salaam early in 1879, but before it reached its first objective point—Nyasa—its leader died. Thomson was then little over twenty-one years of age; but he rose to the occasion, took command, and single-handed carried the expedition to a triumphant conclusion. He explored the plateau between Nyasa and Tanganyika, and the western shore of the latter from its southern end to the Lukuga; there he added another to the pile of contradictory statements as to the relations of this river and the lake. He tried to work westward to the Upper Congo, but, owing to the hostility of the natives, he was compelled to return to Ujiji and back to the coast. This was Thomson's first expedition, and in some ways it was his best, for his scientific observations were then made with greater care and detail than in any of his later journeys. The following year he returned to East Africa to search for the coal reported on the Rovuma. Next year he was sent to Africa on the famous expedition, the story of which he so brilliantly told in "Through Masai-land." He left Mombasa in 1882 with a powerful caravan, fitted out by the Geographical Society, in order to determine whether there be a practical route across the Masai country to the Nyanza, to explore Mount Kenya, and to study the meteorology, ethnology, and natural history of the region traversed. After great difficulties with his men, he marched inland to Taveta, at the foot of Kilima Njaro. There he joined a powerful caravan under the famous slave-trader, Jumbo Kinameta, and together they traversed Masai-land to Lake Naivasha, going first along the route of Last, and then along that of Fischer. Thomson then turned to the east, and was the first European to set foot on the plateau of Laikipia and to see Kenya from the west. But the Masai were present in force, and Thomson had either to fight or retreat. He chose the latter alternative, and, contenting himself with a distant view of Kenya, under cover of night fled northward to Baringo. He explored this district, which he was the first European to reach, and then went on to the Nyanza, and back to the coast. His next expedition was up the Niger. His tact and patience in dealing with natives, here stood him in good stead, and rendered this expedition his most successful, for he returned with the treaties which gained for England practical supremacy in the Niger Basin. In 1888, with Mr. Crichton-Browne, he undertook a journey to south-western Morocco, materially correcting some previous descriptions of the structure of that country. He took a series of altitudes, and with characteristic acumen discovered for himself the divergences between the results given by aneroids and boiling-point thermometers; but it was equally characteristic that he did not follow up the subject, and contented himself with attributing it to the imperfection of his instruments. In 1891 he was sent by the British South Africa Company to annex the metalliferous region of Katanga. He was greatly hindered by the Portuguese, who fired upon his flotilla, and when he reached the frontier of Katanga he found that Captain Stairs's expedition had arrived before him, and secured the country for the Belgians. Thomson returned to England with his health ruined by his six African expeditions. Residence at Kimberley saved him for a while, but phthisis had taken too firm a hold to be dislodged, and after a lingering illness he passed away on August 2.

It is too early to attempt to estimate fairly Thomson's work as an explorer; but no one could follow in his footsteps without recognising how singularly keen was his topographic insight, how rapid his powers of observation, and how marvellously true were his instincts. His powers, in fact, amounted almost to genius. In his quickness of perception and his literary skill he reminds us of Burton, though without Burton's scholarship and colossal capacity for steady work. But Thomson's brilliant gifts had their dangers, and it is impossible to compare his work with that of some of his contemporaries, or even of some of his predecessors, without recognising that he was

sometimes as careless as he was capable, and that he rarely used his great abilities to the full. He belonged to the school of explorers who prefer rapid traverses and pioneer work, to scientific investigations and detailed surveys. He reminds us by his geographical work of Livingstone, and by his love of sport of Selous, rather than of men like Fischer, Schweinfurth and Junker. He was fonder of the field than of the library, and often did not, apparently, know which of his results were new, and which were known before. The thing of which he was proudest was that he had never taken the life of a native, for he had avoided hostilities owing to his tact and infinite patience, which was especially creditable to a man of such an impulsive temperament. His love of peace, however, was not due to any fear of war, for he was brave to recklessness. That he felt warmly, and could speak impatiently, was shown by his criticisms upon the management of the Emin Relief Expedition. In his most famous expedition it was unfortunate that he followed such a trained naturalist and learned ethnographer as Fischer, and was himself followed by such a laborious and skilled cartographer as von Höhnel. On the other hand, this journey was the one which showed Thomson's powers at their best; for he then had the fullest scope for the exercise of his tact as a leader of men, his dash and daring as an explorer, his enthusiasm as a sportsman, and the consummate skill with which he gained the affections of his men and the confidence of suspicious natives. The same qualities won him respect at home. He is described, by those who knew him, as singularly modest and unassuming. His frank sincerity and genial humour endeared him to a wide circle of friends, who devotedly cared for him in his long illness, and now mourn his early death.

J. W. GREGORY.

WILLIAM CRAWFORD WILLIAMSON.

WHEN the author of this article began the work for his "Einleitung in die Palæophytologie," he soon realised that it was quite impossible to produce such a book without an accurate knowledge of Williamson's collection of sections. He therefore wrote to Manchester and requested permission to make use of the collection. An invitation to Williamson's hospitable house was the immediate result. He there spent eight delightful and busy days, during which the host was never weary of demonstrating his specimens to his guest, who was astonished at their abundance, or of imparting to him the fullest information from his store of knowledge. The guest departed with feelings of the warmest respect and gratitude. In the course of the following years, however, he has often again had the privilege of returning to Manchester and London, and of knitting closer the bonds of reverence and friendship with him who is gone. The last occasion was in the spring of the current year, when the writer left with the conviction that it had been their last meeting. Williamson's death actually took place at Clapham Common, on June 23, when in his seventy-ninth year.

William Crawford Williamson was born at Scarborough, on November 24, 1816. His father, John Williamson, a gardener by profession, but by the bent of his mind a naturalist, and especially a geologist, was a zealous colleague of William Smith, who was attached to him both by friendship and by their common pursuits, and who spent two whole years, 1826–1828, under his roof.

Young Williamson's father encouraged his scientific tastes, even from his earliest days; his observational faculties were strengthened by frequent excursions; the association with Smith, and with the circle of active geologists of that fruitful period, influenced his boyhood, and left behind an effect which lasted his whole life. He has often told the writer about his geological and botanical rambles with his father and friends along the beautiful cliffs of the Scarborough and Whitby coasts. He had an extraordinary love for his more immediate home, and was proud to call himself a Yorkshireman.

Williamson's first publications, "On a Rare Species of *Mytilus*," and "On the Distribution of Organic Remains in the Lias Series of Yorkshire," appeared when he was only in his eighteenth year. About the same time he also contributed a considerable number of drawings to Lindley and Hutton's "Fossil Flora of Great Britain," a work which was completed in 1837, when he was twenty-one. In his later years he did not continue to work much at remains preserved as impressions, for his whole interest had become diverted to anatomical studies. One or two papers on *Zamia gigas* (now called *Williamsonia*), however, owe their origin to the material accumulated in those youthful days. The last and most important of these papers appeared in 1870, in the *Transactions* of the Linnean Society, vol. xxvi., under the title "Contributions towards the History of *Zamia gigas*."

Williamson's family was not much blest with this world's goods. He was therefore obliged to adopt some practical calling, and naturally chose the medical profession, for which he prepared, first at Manchester, while at the same time acting as Curator of the Natural History Museum there, and subsequently in London. In 1840 he became member and licentiate of the Royal College of Surgeons. Soon afterwards he settled in Manchester as a medical man, and remained there over fifty years, carrying on for a long time an extensive practice. In addition to this the professorship of Geology and Natural History at the Owens College was conferred on him in 1851, an office which he administered, in its full extent, for many years. In 1872, however, he handed over the geology to Boyd Dawkins, and from 1880 onwards gave up the zoology, and confined himself to botany. This he continued to teach down to 1892, when his decreasing bodily strength compelled him to retire altogether. He then removed to London, in order that with the aid of the greater facilities there offered he might the better advance the scientific work, which he was still zealously pursuing. Here, after three more years, he too soon ended a life of which one may certainly say, with the Psalmist, that its strength was labour and toil.

For medical practice and professorial duties, though strenuously and most conscientiously carried on, did not satisfy Williamson's mighty power of work. Concurrently with these occupations, a constant flow of scientific production went on, the many-sidedness of which is scarcely conceivable to the present generation. Not only did he write articles in medical journals, which lie beyond the scope of the present notice, he also continued to work with the greatest zeal at zoology, botany, and, above all, geology and palæontology, as is testified by his numerous publications—large and small.

From his youth upwards, Williamson had been much occupied with the investigation of fossil fishes, and in the latter half of the thirties, and beginning of the forties, he wrote various memoirs on this subject. His studies of lower organisms gave rise to the works on *Campylodiscus*,[1] on *Volvox Globator*,[2] and on *Foraminifera*, the last and most important of which, embracing the whole of his researches on the subject, was published by the Ray Society in 1858, under the title of "The British Foraminifera." These writings have received due acknowledgment in the works of Carpenter and Bütschli.

In 1833 the remarkable work by Witham, of Lartington, appeared, in which the study of the internal structure of carboniferous fossil plants was entered upon for the first time, with the help of the thin ground sections

1 "Annals of Nat. Hist." vol. i., 1848.

2 *Memoirs* of the Manchester Lit. and Phil. Soc., vol ix. 1851, and *Transactions* of the Microscopical Soc., vol. i., 1853.

invented by Nicol. This work laid the foundation of our knowledge of the structure of the *Lepidodendra* and *Stigmariæ*, and Brongniart then applied the new method, with the most brilliant success, to the investigation of *Sigillaria*. Williamson also soon attained brilliant results by its aid, studying the shells of Foraminifera, and the scales and teeth of fishes. Two papers, published in the *Philosophical Transactions* (1849 and 1851), and considered excellent by competent judges, were the result.

Naturally, the study of fossil plants, which had been so successfully begun, was not neglected, whenever such material could be obtained in the proper state of preservation, which at that time was not easy. Williamson's first attempt of the kind, the precursor of the whole palæobotanical literature which he created, was the paper "On the Structure and Affinities of the Plants hitherto known as *Sternbergiæ*," in which the *Sternbergiæ* were identified as medullary casts, which had been surrounded by an Araucarian wood. As already mentioned, however, the material for an extended use of the method was at that time still wanting. Then, just at the right moment, came the discovery of the calcareous nodules, enclosing vegetable remains, in the Ganister beds of the coal-fields of Lancashire and Yorkshire. The investigation of the treasures thus revealed was first taken in hand by their discoverer, Binney himself, and subsequently by Carruthers and Williamson. The latter first began with works on the Calamarieæ, three of which appeared in rapid succession from 1869 to 1871. They are: "On the structure of an undescribed type of *Calamodendron* from the upper coal-measures of Lancashire"; "On a new form of Calamitean strobilus"; and "On the organisation of an undescribed verticillate strobilus from the lower coal-measures of Lancashire."

As was necessarily the case, material now began to accumulate in Williamson's hands, and he enjoyed the active co-operation of various zealous collectors. Then, in his fifty-fifth year, he began the great series of memoirs which mark the culminating point of his scientific activity, and which will assure to him, for all time, in conjunction with Brongniart, the honourable title of a founder of modern Palæobotany.

In the course of the following twenty years, nineteen memoirs of this series appeared in the *Philosophical Transactions*, under the general title "On the Organisation of the Fossil Plants of the Coal-measures." They all contain exclusively his own observations, made entirely on material from the British coal-fields. It is a gigantic work, which by itself alone would form the abundant fruit of a man's whole life. It was supplemented, however, by various other contributions to the same subject, published in the *Memoirs* of the Literary and Philosophical Society of Manchester, the *Annales des Sciences naturelles*, and the *Annals of Botany*. During the same period, in 1887, also appeared Williamson's exhaustive "Monograph on the Morphology and Histology of *Stigmaria ficoides*," which will long form the basis of our knowledge of these fossils.

The recognition by palæontologists and botanists of the full importance of these works of Williamson's, has been of course a slow and gradual process. This was really due to external circumstances. In the first place, Williamson found it necessary, as the material in his collection, and his own experience increased, to return repeatedly in his later memoirs to plant-remains which had been dealt with in the earlier parts. Consequently, if we wish to obtain an idea of any group, it is always necessary to study several of these treatises simultaneously. This, however, presents great difficulties, except to those who possess separate copies. For the reader stands helpless before a pile of sixteen volumes of the *Philosophical Transactions!*

On the other hand, there is another point which must be taken into account. Williamson's method of anatomical description, clear as it is, bears the stamp of the scholastic ideas of a past time. For this reason it is only understood with difficulty by the botanists of the present day, and must often first be translated into the form now customary. This is laborious, and has stood greatly in the way of the rapid diffusion of his results.

Williamson himself was fully conscious of these drawbacks, and finally, in order to remedy them, he began a new series of memoirs, in conjunction with Dr. Scott, the object of which was to present a connected and systematically-ordered account of the results obtained, clothed in the language of modern anatomy. The first memoir of this series appeared in 1895, in the *Philosophical Transactions*, and treats of the Calamarieæ and Sphenophylleæ. Two further papers are already completed, but he was not spared to see them published.

The basis of all Williamson's labours in fossil botany is, of course, the collection of slides which he left, containing some thousands of preparations. It is unique of its kind in the world, and of the greatest importance, for it contains the evidence for all the innumerable special observations recorded in his works. Like Willdenow's herbarium or Lindley's collection of orchids, it will always remain an invaluable source of information, to which palæontologists from all sides must resort. Its owner was aware of this, and so also is the author of this notice, who may boast that he knows the collection as scarcely any one else does. It was through him that Williamson decided to prepare and distribute, in a printed form, a detailed index, giving exact references to the individual preparations, and the places where they are cited in the memoirs. This was necessary, for the multitude of preparations often made it very laborious, even for the owner, to look out a particular section to demonstrate some special fact. This work was taken in hand about 1890, and has considerably increased the usefulness and value of the collection to posterity. Three instalments, and those the most important, have already appeared under the title, "General Morphological and Histological Index to the Author's Collective Memoirs on the Fossil Plants of the Coal-measures." Only the Cordaiteæ, the Gymnospermous seeds, and a number of fossils of doubtful affinity, are still wanting. We may, no doubt, ultimately look for a synopsis of these from the hand of a friend, so as to complete the entire work.

If we now consider the contents of the palæobotanical literature created by Williamson during the last twenty-five years of his life, we find that it consists, first of all, of the most minute description and reconstruction of all those types of plants which took part in the formation of the coal-beds of Great Britain. He abstained on principle from concerning himself with non-British material. We have acquired from him the most exact knowledge of the structure of the Calamarieæ, the Lepidodendreæ, the Sphenophylleæ, the Ferns, and Lyginodendreæ. As regards several of these groups, it is true, he had before him fairly detailed investigations by previous observers, but in other families, especially the Calamarieæ and Lepidodendreæ, he himself laid almost the whole foundation of our knowledge. He showed that both groups are, as regards their fructifications, indubitable Archegoniatæ, but that they possessed, like our recent Gymnosperms, a secondary formation of wood from a cambium; he taught us to recognise, in the Stigmariæ, the subterranean organs of the Lepidodendreæ and Sigillariæ; he reconstructed in the genera *Lyginodendron* and *Heterangium*, described by him, a type of plant which, by its characters, occupies an intermediate position between Filicineæ and Gymnosperms, especially Cycadeæ. It thus can find no place in the system of recent plants, but represents a direct derivative of the unknown ancestral stock from which the two groups still living have also sprung. In connection with this type, Renault's Poroxyleæ have since turned out to be their

later Permian relations, while the Protopityæ of the Culm are more ancient allies, with similar characteristics. We thus learn how far back we must go, in the series of geological formations, in order to meet with the last traces of the common ancestors of those classes in the vegetable kingdom which are now living.

By his discovery of Archegoniate plants with secondary growth, Williamson however came into collision with the doctrines of Adolphe Brongniart, otherwise so highly reverenced by him, who held this character to be an absolute criterion of the Phanerogams, and denied the possibility of its occurrence in other classes of the vegetable kingdom. Hence a literary feud arose between Williamson and B. Renault, Brongniart's distinguished pupil. The latter endeavoured to prove that Williamson was in error in the identification of his *Lepidodendra*, that they were really *Sigillariæ*, and together with the latter belonged to the Gymnosperms, while the truly Archegoniate *Lepidodendra* were destitute of any secondary growth. The answer was not long in coming; proof was heaped on proof, until ultimately the real state of the case was made completely clear. In all essential points victory was on the side of our author. Other subsidiary differences respecting *Stigmaria*, the Calamarieæ, &c., require no more than a mention here.

It was thus made evident by Williamson that cambial growth in thickness is a character which has appeared repeatedly in the most various families of the vegetable kingdom, and was by no means acquired for the first time by the Phanerogamic stock. This is a general botanical result of the greatest importance and the widest bearing. In this conclusion Palæontology has, for the first time, spoken the decisive word in a purely botanical question. The result has proved well worth the great trouble and labour which had to be gone through in order to attain it.

It would be difficult to conceive a more magnificent monument to Williamson than one which he himself set up at Manchester, in one of the halls of the Owens College Museum.

In the year 1887 there was discovered in a quarry near Bradford, a gigantic petrified tree-stump, which, when carefully exposed, was found to run out at the base into a widely-spreading system of ramifications of a Stigmarian character. In the quarry this precious relic, like many others before it, would in a very short time have fallen a victim to destruction by weather and the hand of man. Williamson, however, acquired it by purchase, had it carefully subdivided into numerous pieces, and brought it home safe and complete to Manchester. This was not accomplished without the greatest personal exertions and a considerable expenditure of money (to which several friends contributed), for there were whole waggon-loads of material to be removed. Then the first thing which had to be done was to secure from the University authorities the necessary space for erecting the fossil. This was not an easy matter, and great opposition had to be overcome, as we can easily understand on looking at the specimen, which measures over 29 feet in diameter.

Finally it was fitted together, piece by piece, and fixed in its natural position, resting on a massive pedestal of brickwork. The fiery youthful zeal of a man already over seventy, overcame all the difficulties that arose. People were astonished at the unusual development of energy which this *Stigmaria* had caused, and gave it, in good-humoured jest, the name of "Williamson's Folly." "Williamson's Folly" may now be reckoned among the sights of England, and Manchester may be proud of possessing it, for it represents a last gift, worthy of all honour, from the deceased, to the place which for so many years was his home and the scene of his activity.

The author of this notice, who only knew Williamson during the last years of his life, must not attempt to picture to those who lived with him his kindly and benevolent nature, which always retained the freshness of youth, or his simple character. That would be a work of supererogation, for the whole of scientific England knew and respected him, and wherever he went he was a welcome and honoured guest. The writer can only report, in all brevity, on the work of Williamson's life, and when asked to undertake this, it was with pleasure that he took up his pen for that purpose.

SOLMS-LAUBACH.

NOTES.

THE resignation of Dr. Albert Günther, F.R.S., of the post of Keeper of Zoology at the Natural History Museum, South Kensington, is announced. Dr. Günther has occupied for over thirty years the position he now vacates.

THE "Swiney" Lecturer this year is Dr. J. G. Garson, who will take as the subject of the twelve lectures he purposes giving, "The Geological History of Man." The lectures, admission to which will be free, are to be delivered in the lecture theatre of the South Kensington Museum on Mondays, Wednesdays and Fridays, at five P.M., beginning on Friday, October 4.

WE have to record the death of two prominent members of the medical profession abroad, viz. Dr. Pasquale Landi, Professor of Clinical Surgery successively in the Universities of Siena, Bologna, and Pisa, and Dr. Texier, Professor of Internal Pathology in the Medical School of Algiers.

MR. CHARLES MITCHELL, whose death, at the age of seventy-five, occurred on August 22, was a well-known engineer and shipbuilder. He founded the Walker shipbuilding yard on the Tyne, a yard which under his guidance developed into one of the largest in the country. In 1882 it was merged into the Elswick Company of the present Lord Armstrong, and up to the time of his death Mr. Mitchell practically superintended the whole of the shipbuilding work of the Company.

THE *Athenæum* says that during the autumn of this year a monument is to be unveiled at Osteel, in East Friesland, in memory of the discoverers of the sun's spots, David and Johann Fabricius. The site chosen is the place in the cemetery where the grave of the elder Fabricius was discovered about nine years ago.

WE are informed by Prof. John Milne, that communications respecting the *Transactions* of the Seismological Society, and the *Seismological Journal*, may be addressed to him at Shide Hill House, Shide, Newport, Isle of Wight, at which place a small station has been established to record earthquakes having their origin in distant localities, and other unfelt movements of the earth's surface.

THE annual general meeting of the Federated Institution of Mining Engineers will be held in North Staffordshire, at Shelton, Stoke-upon-Trent, on September 18 and 19, when papers on "The Depth to Productive Coal-measures between the Warwickshire and Lancashire Coal-fields," "Gold-mining in Nova Scotia," "The Use of Steel Girders in Mines," "Economic Minerals of the Province of Ontario, Canada," and "The Blasting Efficiency of Explosives" are expected to be read, and a discussion of various papers which have already appeared in the *Transactions* of the Institution may take place. A number of excursions are also arranged.

THE fifth quadrennial meeting of the International Congress of Otology will take place at Florence, under the presidency of Dr. V. Grazzi, from September 23 to 26. Various discussions will be opened by Dr. Barr of Glasgow, Dr. Gellé of Paris, Prof. Gradenigo of Turin, Prof. A. Politzer of Vienna, and Dr. Secchi of Bologna; and there are in the complete programme,

which has just been issued, the titles of no fewer than fifty-nine original communications to be brought before the meeting. It is hoped that British otology will be well represented, as it is intended to invite the next congress to meet in London, either in 1898 or 1899. Full particulars as to terms of membership, routes, hotels, &c., may be obtained from Dr. St. Clair Thomson, 28 Queen Anne-street, W.

AN International Congress of Technical, Commercial, and Industrial Education is being organised by the Société Philomathique of Bordeaux, and is to be held at Bordeaux from September 16 to 21. The programme is, we understand, a full one, and contains many items of interest and importance.

A FEATURE of the annual meeting of the Yorkshire Naturalists' Union, which is to take place at York on October 30, will be an exhibition of specimens, photographs, &c., showing work done during the past year in all departments of the Union. It is requested that all members who intend to exhibit will communicate direct with the Local Secretary, at the Museum, York, on or before October 21.

THE various medical schools will be reopened at the beginning of October, and at most of them introductory addresses will be delivered to the students. On October 1, at St. George's Hospital, the speaker will be Mr. George D. Pollock; at the Middlesex Hospital Dr. W. Julius Mickle, and at the Westminster Hospital Dr. Monckton Copeman. At the latter institution Viscount Peel will distribute the prizes. The introductory address at University College will be delivered by Prof. J. Rose Bradford, and the annual dinner of old and present students will take place at the Hôtel Métropole on October 1, under the chairmanship of Sir Richard Quain, Bart. Mr. A. P. Laurie will give the address at St. Mary's, and the annual dinner will be held the same evening at the Holborn Restaurant, Mr. Malcolm Morris occupying the chair. At St. Thomas's Hospital the prizes will be distributed, on October 2, by Sir Edwin Arnold, K.C.I.E. At Guy's there will be no formal introductory address, but on the evening of October 1 Mr. J. De'Ath will read a paper at the opening meeting of the Physical Society, on "Our Profession, our Patients, our Public and our Press." The annual dinner will take place in the Club Dining Hall, Dr. Pye-Smith in the chair. At the Yorkshire College, Leeds, Prof. D. J. Leech will, on October 1, distribute the prizes and deliver an address. Prof. Victor Horsley is announced to speak at the Sheffield School of Medicine, Mr. Jonathan Hutchinson at University College, Liverpool, and Prof. F. H. Napier at St. Mungo's College. At Mason College, Birmingham, Prof. Percy Frankland will deliver the address, taking as his subject "Pasteur and his Work."

THE Council of the Institution of Civil Engineers has issued a list of suggested subjects for papers during the session 1895–96, for which the undermentioned prizes may be awarded: (1) The Telford Fund, left "in trust, the interest to be expended in annual premiums, under the direction of the Council." The bequest (with accumulations of dividends) produces a gross amount of £235 annually. (2) The Manby Donation, of the value of about £10 a year, given "to form a fund for an annual premium or premiums for papers read at the meetings." (3) The Miller Fund, which, with accumulations of dividends, realises nearly £140 per annum. Out of this the Council has established a scholarship called "The Miller Scholarship," and is prepared to award one such, not exceeding £40 in value, each year, and tenable for three years. Competitors for this scholarship must be under the age of twenty-five years. (4) The Crampton Bequest of £500, the annual income of which amounts now to £13 14s., is devoted to the foundation of "The Crampton Prize," for "presentation to the author of the best paper on the Construction, Ventilation, and Working of Tunnels of Considerable Length, or failing that, then of any other subject that may be selected." (5) The balance of the Trevithick Memorial Fund of £100 0s. 9d., the interest of which is £2 15s. a year. The list of suggested papers, although not exhaustive, is far too long for us to print, but may be had, with further information, upon application to the Secretary of the Institution.

THE Royal Academy of Medical, Physical, and Natural Sciences of Havannah, at a meeting held on April 28, decided to offer amongst other prizes, mostly for medical essays, one—the Cañongo Prize, value 250 dollars in gold—for the best essay on "The Pharmacological Study of the Fluid Extracts." The competition, which is open to any person whether belonging to the medical profession or not, will be closed on March 19, 1896, by which date all papers must be sent in, written in French or Spanish, and sealed, with a motto on the internal envelope, and in another envelope bearing the same motto the author's name and address. The adjudication will take place on May 19, 1896, when the prizes will be distributed to the successful competitors. Further particulars may be obtained by writing to the Secretary, Dr. Vicente de la Guardia, Havannah.

UNDER the active presidency of the Earl of Derby, a vigorous effort is being made by the British Dairy Farmers' Association to give a helping hand to one of the most important branches of agriculture, dairy farming, and its allied industry of poultry raising. At the twentieth annual London Dairy Show, to be held at the Royal Agricultural Hall in October next, prizes to the value of £2515, in addition to 142 gold, silver, and bronze medals, are offered for competition in 451 different classes, in many of which a keen contest is already assured.

AN interesting memoir has been recently published by Dr. Max Müller, on the effect of fever temperature upon the growth and virulence of the typhoid bacillus. In view of the conflicting opinions which have from time to time prevailed on the manner in which a high temperature affects the agent of infection in cases of typhoid fever, these results are of some considerable practical interest. Thus in 1882 we find Jörgensen ventilating the idea that the development of the morbific material in the system in cases of typhoid fever might be retarded by greatly reducing the temperature of the body, whilst other authorities have as confidently stated that the feverish rise in temperature was capable of destroying the typhoid organism, or, at any rate, hindering its development. Both of these opinions are based on very slender experimental evidence. Dr. Max Müller has carried out a series of researches in which he has carefully recorded the growth of the typhoid bacillus at various temperatures, and he states that when preserved at about 40° C. this microbe takes five minutes longer to proliferate, or produce a new generation, than when it is kept at a temperature of from 37·5° to 38·0° C. respectively; that is to say, in the absence of all adverse circumstances, under the most favourable conditions, as many as forty-five generations of typhoid bacilli may proceed in one day from a single parent bacillus at the *normal* temperature of the body, whilst at about 40° C. thirty-nine such generations may be elaborated. In considering these appalling figures it must, however, be remembered that such an uninterrupted multiplication of the typhoid bacillus does not necessarily take place in the human system; the conditions which surround it in the latter case are of a far more complicated and subtle character than those which obtained in Dr. Müller's laboratory culture-tube! But these results show that a fever temperature of about 40° C. is not able to destroy the typhoid bacillus, or to affect its growth to any considerable extent; even higher temperatures of 41·5° to 42·0° C. were also incapable of annihilating this microbe, and typhoid bacilli kept for sixty-two days at 42·0° C. showed subsequently no abatement of their vitality. As regards

the effect of such temperatures on the virulence of the typhoid bacillus, Dr. Müller states, but only as the result of very limited experiments, that he could detect no difference in the behaviour in this respect of those kept at 37° and 40° C. respectively.

A MODIFIED centesimal system of subdividing time and angular measures is advocated by M. H. de Sarranton, in the *Revue Scientifique*. He proposes to retain the hour as a fundamental unit of time, on account of its universal acceptance, its convenience, and the hopelessness of the task of altering it. But the hour should be divided into 100 minutes, and the minute into 100 seconds. Thus each new minute would be three-fifths of an old minute, or thirty-six seconds, while the new second would be a little over a third of the present second. Two of the new seconds would cover the time of a brisk step, like the accelerated pace used in the French army. The new second is the time taken by one semi-vibration of a simple pendulum 12·9 cm. long. Time could then be consistently expressed in hours and decimals. Thus 8·3348 h. might be read 8 hours 33 (new) minutes 48 (new) seconds, and calculations involving time would be much simplified. Clock and watch dials would be subdivided into hours, as usual, but the smaller divisions for the minute and seconds hands would be hundredths of the circle instead of sixtieths, and every tenth division would have to be slightly marked. For angular measurement, M. de Sarranton proposes 240°, subdivided into 100 minutes of 100 seconds each, so that they could be converted into hours by shifting the decimal point one place to the left.

A FEW particulars of the new mouth of the Vistula are given in the *Globus*. It was made by regulating the old branch going into the Baltic, which was straightened and shortened from ten miles to four and a half, while the channel was broadened by shifting the dyke on the left bank six miles to the west. At the same time, the Danzig branch was cut off by a lock. This useful piece of work will not only make the Vistula more accessible, but will prevent the disastrous floods which caused far-reaching destruction in winter and spring, near the mouth of the river. The work cost a million pounds, half of which was borne by the districts concerned, and half by the German Treasury.

THE current number of the *British Medical Journal* has a note on the vision of School Board children, based upon a report of Dr. James Kerr, medical superintendent of the Bradford School Board. The tests employed were designed to detect every child who had not good distant vision with one eye at least, the list of children thus obtained including those with defect of distant sight from all causes, remediable or otherwise. Such a list having been made, it was an easy matter to more fully examine all the children thus tabulated, and to classify and deal with them as might be necessary. In the report, tables are given showing the number of children examined, and the percentage of defective eyesight in the different standards from one to seven. A perusal of Dr. Kerr's report will, in the opinion of our contemporary, well repay those who have to conduct similar examinations of large numbers of school children.

WE have received from the Deutsche Seewarte (Hamburg) the report of its labours during the year 1894. The duties of this institution differ materially from those of the German Meteorological Institute (Berlin), whose report we lately noticed, inasmuch as the former deals specially with weather prediction and marine meteorology. In both of these branches great activity is shown, and we have frequently referred to the useful work carried on. The detailed discussion of the meteorology of the various oceans, for the benefit of seamen, the preparation of synoptic weather charts of the North Atlantic Ocean, for the advancement of practical meteorology, and the publication of observations taken in remote parts of the world, are noteworthy instances of the industry of the institution. For the purpose of obtaining information relating to maritime meteorology, it has not only established many agencies in German ports, but the Consuls in several foreign ports, including English, also take part in enlisting observers and supplying the necessary registers; the result being that about 450 voluntary observers were co-operating at the end of the year in the mercantile marine alone.

THE annual report of the Department of Mines and Agriculture, New South Wales, for the year 1894, has come to hand. In it reference is made to the resignation of the position of palæontologist of Mr. Robert Etheridge, occasioned by his accepting the curatorship of the Australian Museum. Mr. Etheridge will, however, we are pleased to notice, still retain connection with the department, having, the report says, volunteered to act as honorary consulting palæontologist.

WE have received from the Keeper of the Manchester Museum, Owens College, a new handy guide to the museum, which has been compiled for the purpose of indicating cursorily the principal objects in the building and its general arrangement, for the benefit of visitors whose time is limited. To those who can afford time to pay several visits, the illustrated guide is recommended as being more complete and useful.

THE new part of the *Asclepiad*, Sir B. Ward Richardson's quarterly, contains articles on "Cycling and Heart Disease," "The late prevailing Epidemic," and, with portrait, "John Abernethy, F.R.S."

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*, ♂ ♀) from India, presented by Mr. Hugh H. Collis; a White-tailed Sea-Eagle (*Haliaetus albicilla*) from Northern Russia, presented by Mr. Robert Ashton; two Red-backed Shrikes (*Lanius collurio*), British, presented by Mr. C. Ingram; a Natterjack Toad (*Bufo calamita*) from Surrey, presented by Mr. Hanley Flower; a Melodious Jay Thrush (*Leucodioptron canorum*), deposited, a —— Capuchin (*Cebus ?* ♂), a Porto Rico Pigeon (*Columba corensis*), a Vinaceous Pigeon (*Columba vinacea*), a Barn Owl (*Strix flammea*), seven Adorned Ceratophrys (*Ceratophrys ornata*) from Brazil, purchased; a Great Kangaroo (*Macropus giganteus*, ♂), a Rufous Rat Kangaroo (*Hypsiprymnus rufescens*, ♂), a short-headed Phalanger (*Belideus breviceps*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE FORMS OF JUPITER'S SATELLITES.—A paper, by Mr. S. I. Bailey, on the forms of the discs of Jupiter's satellites, is communicated by Prof. E. C. Pickering to the current *Astrophysical Journal*. A number of observations of the satellites has been made with the thirteen-inch refractor at the Arequipa Observatory; and the results were: "Under the best conditions, that is, with the instrument in perfect adjustment and good seeing, satellites II., III. and IV. were always seen round. Satellite I. was twice seen having an apparent elongation in the same direction as Jupiter. In both cases the satellite was near the planet. On the second occasion, I., when off the disc, but near Jupiter, appeared elongated, but an hour later, plainly seen on the disc of Jupiter, it appeared perfectly round. On the other hand, the shadows of I. and III. on other nights were seen elongated. Several occultations and transits were observed, but the limb of Jupiter was not seen when, to me, it gave any indication of transparency. . . . During the hours given, we failed to detect any systematic change of form in any of the satellites. These observations, scattered through the cloudy season, may not be the best possible, for the same observers and instrument in Arequipa, nevertheless it does not seem probable to me that any frequent periodic recurrence of an ellipticity, approximating in amount that of Jupiter itself, would have escaped detection."

EPHEMERIS OF SWIFT'S COMET.—The following elements and ephemeris of Swift's comet, the reappearance of which was announced last week, have been computed by Dr. Berberich, and are published in *Edinburgh Circular*, No. 45. The elements are deduced from the observations:—Mount Hamilton, August 21; Nice (M. Juvelle), August 24; Hamburg, August 25.

Another observation of August 23, made by Mr. J. Witt at the Urania Observatory, Berlin, is closely represented by the ephemeris. Dr. Berberich thinks the comet will possibly belong to the group of periodic comets with short revolution.

Elements.

T = 1895, Sept. 3·3630, M.T. Berlin.

$$\left.\begin{aligned} \omega &= 179^\circ\ 37'\cdot94 \\ \Omega &= 172\ \ 59\cdot65 \\ i &= \ \ 4\ \ 38\cdot55 \end{aligned}\right\}\ \text{Mean Equinox } 1895\cdot0.$$

$$\log q = 0\cdot16537.$$

Ephemeris for Berlin Midnight.

1895.	R.A. h. m. s.	Decl. ° ′	log. Δ.	log. r.	Brightness.
Sept. 4	1 0 7	+6 17·3			
6	1 4 3	6 18·0	9·7100	0·1656	1·17
8	1 7 50	6 17·5			
10	1 11 28	6 15·9	9·7071	0·1664	1·18
12	1 14 57	6 13·2			
14	1 18 16	6 9·6	9·7062	0·1679	1·18
16	1 21 25	6 5·1			
18	1 24 23	5 59·7	9·7073	0·1700	1·16
20	1 27 11	5 53·6			
22	1 29 49	+5 46·9	9·7107	0·1727	1·16

The brightness at August 21·5 has been taken as unity.

COMETS AND THE SUN-SPOT PERIOD.—Since the discovery of the periodicity of the sun-spots, investigations have shown that many terrestrial phenomena are, and others may be, closely allied to it. These are generally looked upon as results due to the variation in the sun. If it were found that comets had an eleven-yearly period, we should have the question before us as to whether this period be the result of this period, or whether the period depended to a certain extent on this periodical cometary influx. If the sun, as has been supposed, were fed, so to speak, with cometary matter, then the spot period would naturally be dependent on some external source of supply such as this. But since the solar atmosphere has a circulation which seems now to have been fairly shown to be the cause of this periodicity, such an outward supply of energy is not thought now to be of such importance as would have been the case some years ago. This does not take away the interest, however, from Herr J. Unterweger's investigation concerning the connection of spots and appearances of comets, but would rather instigate it. The author has, by a strict examination of the elements of the larger periodical comets, obtained a function which can be represented mathematically by a formula, and from which an eleven-yearly period since 1740 can be recognised. From the year 1833, also, the maxima and minima points fall together, without exception, with those of the sun-spot curves. In determining the length of the period, the amplitude of the period was set for each series as a function of the length of the period, and then each value for the duration of the period ascertained. The calculation was so arranged that two neighbouring values, which made the amplitude a minimum, were also determined. The values for the function came out as 8·682, 11·226, 13·365 years, those for the series showing the relative number of sun-spots being 8·721, 11·254, 13·424 years.

To determine also whether the points of maxima and minima for the function were coincident with those for the series showing the sun-spot numbers, the curves drawn from the values derived coincided to such an extent that a secondary maximum could be recognised on both of them in similar positions.

Other results which the investigator indicates as having been shown are:—The identification of the thirty-five-yearly sun-spot period, the function giving larger values in 1778, 1816, 1848 and 1882, and smaller minima in 1764, 1806, 1834 and 1867; the time between two successive maxima being in the mean 34·8 years.

The secular period 1764–1806, with maximum at 1777–80, coincides with a secular maximum of sun-spots and a large *gletscherverstors* which began in 1768 and ended about 1785. The 1806–1834 interval, with a maximum at 1816, corresponds to a maximum of sun-spot and to an intense *gletscherverstors* from 1814 to 1824. The third and fourth periods are also likewise explained.

Cases are also made out for the secular variations in the climate, and a suggestion is thrown out that if we may look upon "Kometen als stark elektrische Massen," then at the times of their maximum number and least distances from the earth, small induced currents may be set up, which will be recorded by the magnetic needle; this latter question has not, however, been investigated.

THE SUN'S PLACE IN NATURE.[1]

XI.

The Clock Rate.

THE proper regulation of this clock error and consequent "trail" of the spectrum across the plate parallel to itself are essential to the success of photographs taken by the objective prisms. The spectrum of a bright star must obviously be made to trail more quickly than that of a fainter one, and a shorter exposure is sufficient. Since for the same clock error, and in the same time, a star near the pole will give a shorter trail than one nearer the equator, declination must also be taken into account. Keeping a constant clock error, equal widths of spectrum for stars of different declinations may be obtained by lengthening the time of exposure for stars away from the equator, but in that case, the stars near the pole would be over-exposed in relation to those nearer the equator.

The exposure given to stars of equal magnitudes should evidently be the same, no matter in what part of the sky they may be situated, and the clock error should, therefore, be increased in proportion to the secant of the angle of declination.

The light-ratio of stars being $2\cdot512^n$, where n expresses the difference in magnitude, the time of exposure must vary in the same proportion, and the clock error in inverse proportion. Thus, where 5 minutes' exposure is sufficient for a first-magnitude star, 31 minutes is required to obtain a fully-exposed spectrum of a star of the third magnitude. This law, however, only applies to photographic magnitudes, and must be modified according to the type of spectrum or the colour of the star.

The red stars, being much weaker in blue and violet rays than the yellow or white stars, require much longer exposures than white stars of equal magnitude. To obtain a spectrum of β Pegasi extending to the K line, for example, at least three times the exposure required by a white star of similar magnitude must be given.

For conveniently adjusting the exposures, tables have been constructed which show at a glance the position of the regulator for a star of given magnitude and declination.

It is obvious that with an instrument of high dispersion, the number of stars it is possible to photograph is very limited, as the long exposures required for the fainter stars are impracticable, and, even if possible, the definition of the lines would be destroyed by atmospheric tremors.

Hence, it is at present only possible to photograph the spectra of the faint stars on a very small scale. With an objective of 8 inches aperture and 44 inches focal length, and a prism of 13° refracting angle, Prof. Pickering has photographed the spectra of stars down to the eighth magnitude. These spectra are about 1 centimetre long, and a millimetre broad, and though they do not show a very great amount of detail, they are sufficient to reveal the type of spectrum.

With an instrument capable of photographing faint stars, a large number of spectra may be taken at one exposure; but, with the instruments of larger dispersion, this is not generally the case, as there are few bright stars of nearly equal magnitude sufficiently close together.

The Electrical Control.

In consequence of the great accuracy required in the driving of the telescope when long exposures are necessary, the 10-inch equatorial has been fitted with a simple and inexpensive form of electrical control. This is a modification of that designed by Mr. Russell, of the Sydney Observatory.[2] The existing driving

[1] Revised from shorthand notes of a course of Lectures to Working Men at the Museum of Practical Geology during November and December, 1894. (Continued from page 425.)

[2] *Monthly Notices*, vol. li. p. 43, 1890-91.

gear has been altered so that the driving rod performs its revolution in a second, and the motion is then communicated to the driving screw through a small worm wheel. The driving rod is vertical and in two parts, the lower portion ending in a faced ratchet wheel, 3 inches in diameter, and with 200 teeth. The upper part of the rod ends in an arm at right angles to itself, and this arm carries a ratchet of suitable shape held down by an adjustable spring. An electro-magnet connected with the controlling pendulum, is arranged so as to only permit the ratchet to pass it once a second (see Fig. 42). If the clock be driving too

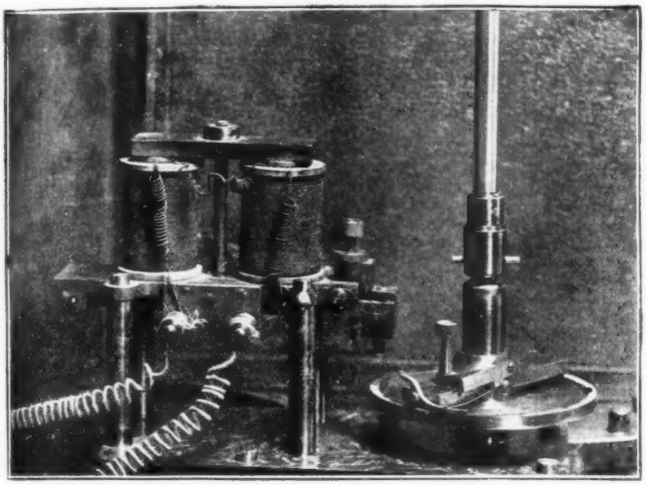

FIG. 42.—Electrical control for 10-inch equatorial.

quickly, the ratchet is held until the stop is raised by the pendulum. When held in this way the ratchet is lifted out of the teeth, and the driving clock itself is not affected.

In order that this form of control may be effective, it is essential that the clock should be going too quickly, as it is only capable of retarding the driving-rod.

The controlling pendulum is, of course, regulated to the rate required for the particular star which is being photographed.

In Mr. Russell's form of control the two parts of the driving rod are connected by friction plates. It was found, however, on testing this arrangement, that when the upper portion was held by the electro-magnet the rate of the governors was seriously retarded; hence I introduced a ratchet wheel, and its working leaves nothing to be desired.

Enlargements of the Negatives.

Many of the negatives taken have been enlarged about nine times on glass, and further copies have been taken on bromide paper, bringing the enlargement up to about twenty-five times the size of the original.

Owing to various causes the photographic spectra obtained by the method of trails show irregularities resembling the lines along the spectrum observed when the slit of a spectroscope is partly clogged with dust. It has been noticed that the period of the irregularities is equal to the time of revolution of the main driving screw of the telescope, and hence they may be accounted for by supposing the driving gear to be mechanically imperfect. In that case some of the parallel lines which, by their juxtaposition form the broadened spectrum, are superposed, while others are drawn apart, thus giving rise to dark and bright lines parallel to the length of the spectrum. These lines are more apparent in the case of bright stars than fainter ones. If the telescope were driven with perfect regularity and the atmosphere were quite steady, we should obtain a spectrum of uniform intensity along its width. This condition has very nearly been obtained in some cases.

The irregularities above described are eliminated in the enlarged negatives by giving them a very slight up-and-down motion during exposure in a direction parallel to the lines of the spectrum. This was originally done by hand, but a negative holder has been constructed in which the necessary motion is given to the negative by a small driving clock.

A diagram of the arrangement is given below. The only drawback to this method is that defects of the film are apt to produce, by a succession of their images on the enlarging plate, lines (generally very faint) which have a semblance of the true spectrum lines.

To distinguish the real lines from the artificial ones, a direct enlargement of the spectrum is made on the same plate alongside the other, the to-and-fro motion being dispensed with. By a comparison of the two enlarged strips, one can see at a glance which are the true lines of the spectrum, and which are those produced by small irregularities on the film. It may be stated that Dr. Scheiner has also used a somewhat similar method to the one described, the only difference being that he caused the plate on which the enlargement was to be taken to have the oscillating motion, instead of the original negative. The method employed by me, though no account of it had been published, had been in use for some time before Dr. Scheiner's method was announced.[1]

My object was not so much to obtain photographs of the spectra of a large number of stars, as to study in detail the spectra of comparatively few; hence many of the stars have been photographed several times with special exposures and foci for different regions of the spectrum.

As in the case of stellar spectra observed by eye, the photographic spectra vary very considerably in passing from star to star.

In the classification of stars adopted from a consideration of the visual observations, only the broader differences in the spectra have been taken into account. Prof. Pickering, however, has suggested a provisional classification in connection with the Henry Draper Memorial photographs of stellar spectra, but this chiefly relates to photographs taken with small dispersion. Now that it has become possible to obtain large dispersion photographs of the spectra, much more detail is revealed, and hence I determined to deal with the presence, or absence, or changes of intensity, of individual lines to a greater extent than Prof. Pickering has done in his observations so far published.

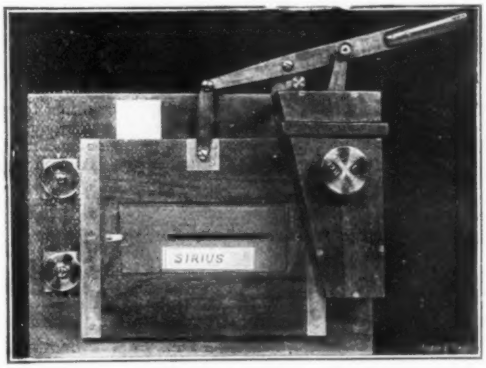

Fig. 43.—Negative holder used in enlarging.

In the first instance, I arranged the various stars of which the spectra have been photographed in tables, without reference to any of the existing classsfications, and not taking into account the finer details.

The basis upon which this first grouping was founded is the extent of the continuous absorption at the blue end of the spectrum. Such a distinction was not possible in the case of

[1] NATURE, vol. xlii. p. 303, 1890.

eye observations, and it is only by photographs that a classification from this point of view can be made.

Some spectra show a remarkable continuous absorption either in the ultra-violet or violet, in others this absorption extends to about K, whilst in a third class it reaches as far as G.

These considerations gave four marked groups. Each of these main groups are next sub-divided into sub-groups by the most marked differences in the spectral lines. I do not propose to give the detailed inquiry in this place.

The important fact which stood out when the photographic attack had got so far was that, whether we take the varying thicknesses of the hydrogen lines or of the lines of other substances as the basis for the arrangement of the spectra, it was not possible to place all the stars in one line of temperature, but it was necessary to arrange the stars in two series.

When this sorting was completed, I was in a position to consider the various divisions of the photographic spectra thus arrived at, in relation to the groups which were previously suggested from a discussion of eye observations. It is clear that if I got the same results the first conclusions would be strengthened.

We have, therefore, to inquire how far this condition is satisfied by the mass of new facts at our disposal. This involves the consideration of some points in connection with the meteoritic hypothesis, and it must specially be borne in mind that the fundamental difference between mine and other classifications is that it demands the existence of bodies of increasing as well as bodies of decreasing temperatures.

Since in my classification the connection between nebulæ and stars is insisted on, it was necessary to obtain a spectrum of one of the brightest of the nebulæ as a term of comparison. The nebula of Orion was selected, and a photograph taken with a 30-inch silver on glass reflector in February 1890. This photograph contained 54 lines, which were carefully tabulated for the purposes of the comparison to which reference has been made.

The Complex Origin of the Spectra of Nebulæ.

On the hypothesis, the bright lines seen in the nebulæ should have three origins.

(1) The lines of those substances which occupy the greatest volume (or largest area in a section); in other words, the lines of those substances which are driven furthest out from the meteorites and occupy the interspaces, when possibly they may be rendered luminous by electricity. Chief among these, from laboratory experiments, we should expect hydrogen, and next, from the same experiments, we should expect gaseous compounds of carbon.

(2) We are justified in assuming that the most numerous collisions will be partial ones—grazes—sufficient only to produce comparatively slight rises in temperature. The nebula spectrum, so far as it is produced by this cause, will therefore depend upon the phenomena produced in greatest number, and we may hence expect to find the low temperature lines of various metallic substances.

(3) In addition to the large number of partial collisions there will be a relatively small number of end-on collisions, producing very high temperature,[1] and, so far as this cause is concerned, there will be some lines produced which are associated with very high temperatures.

Combining these conclusions, in the spectra of nebulæ we should expect to find evidence of

Hydrogen and compounds of carbon.
Low temperature metallic lines and flutings.
Lines which are only produced at very high temperatures.

The Passage to Bright-line Stars.

On the hypothesis, the lines seen in the spectra of bright-line stars should, in the main, resemble those which appear in nebulæ. They will differ, however, for two reasons:—

(1) Owing to partial condensation of the swarm the hydrogen area will be restricted, and the bright lines of hydrogen will lose their prominence; the volume occupied by the carbon compounds will be relatively increased, and the brightness of the carbon bands will be enhanced.

(2) On account of the increased number of collisions, more meteorites will be rendered incandescent, and the continuous spectrum will be brighter than in nebulæ.

[1] *Roy. Soc. Proc.*, vol. xliii. p. 150.

Stars of Increasing Temperature.

Initially, each pair of meteorites in collision may be regarded as a condensation.

Ultimately, when all the meteorites are volatilised, there will only be one condensation, in the shape of a spherical mass of vapour. Between these points there must be other conditions.

(Stage 1.) At the stage of condensation immediately following that of the bright-line stars, the bright lines from the interspaces will be masked by corresponding dark ones produced by the absorption of the same vapours surrounding the incandescent meteorites. One part of the swarm will give bright lines, another dark lines at the same wave-lengths, and these lines will therefore vanish from the spectrum. The interspaces will be restricted so that absorption phenomena will be in excess, and the first absorption will be that due to low-temperature vapours, that is, fluting absorptions of various metals. The radiation spectrum of the interspace will now be chiefly that of the compounds of carbon. Under these conditions we know from laboratory experiments[1] that the amount of continuous absorption at the blue end will be at a maximum.

(Stage 2.) With further condensation the radiation spectrum of the interspaces will gradually disappear, and the fluting absorptions will be replaced by dark lines, for the reason that the incandescent meteorites will be surrounded by vapours produced at a higher temperature, the number of violent collisions per unit time and volume being now greatly increased. This dark line spectrum need not necessarily resemble that of the Sun.

(Stage 3.) The line absorption and the continuous absorption at the blue end of the spectrum will diminish as the condensations are reduced in number, for the reason that only those vapours high up in the atmospheres surrounding the condensations will be competent to show absorption phenomena, in consequence of the bright continuous spectrum of the still disturbed lower levels of those atmospheres.

Among the more important lines which will disappear at this stage will be those of iron, for the reason that there will be bright lines from the interspaces occupying the same positions as the dark lines produced by the absorption of the vapour surrounding the stones.

The number of violent collisions per unit time and volume being further increased, we should expect the absorption of very high temperature vapours.

The Hottest Stars.

Ultimately, then, we should expect that the order of the absorbing layers will follow the original order of the extension of the vapours round the meteorites in the first condition of the swarm, and the lines seen bright in nebulæ, whatever their origins may be, should therefore appear almost alone as dark lines in the hotter stars, and the hydrogen especially should have its lines broadened with each increase of depth in the atmosphere. The continuous absorption at the violet end of the spectrum will be at a minimum. If, when the hydrogen lines are thick the swarm is not yet completely condensed, that is, if there be nebulous matter surrounding the central mass of vapour, a fine bright line will be seen down the centre of each dark one.

Stars of Decreasing Temperature.

When we consider the cooling condition, that is, what happens when the temperature of the mass of vapour is no longer increased by the fall towards the centre of meteorites composing the initial swarm, we should expect to find the phenomena indicated below.

(Stage 1.) The hydrogen lines will begin to thin out, on account of the diminishing depth of the absorbing atmosphere, and new lines will appear.

The new lines will not necessarily be the same as those observed in connection with the stars of increasing temperature.[2] In the latter there will be the perpetual explosions of the meteorites affecting the atmosphere, whereas in a cooling mass of vapour we have to deal with the absorption of the highest layers of vapours. Those lines which will first make their appearance, however, will be the longest low temperature lines of the various chemical elements.

[1] Lockyer and Roberts-Austen, *Roy. Soc. Proc.*, 1875, p. 344.
[2] *Roy. Soc. Proc.*, vol. xlv, p. 382.

(Stage 2.) The hydrogen lines will continue to thin out, and when the absorption of the hotter lower layers makes itself felt the spectra will show the high temperature spectra of the various chemical elements, showing many more lines. The difference between these and the lines seen in stars of increasing temperature should be one due to the different percentage composition of the absorbing layers, so far as the known lines are concerned.

With this increasing line absorption there will be a recurrence of the continuous absorption in the ultra-violet.

(Stage 3.) With the further thinning of the hydrogen lines and reduction of temperature of the atmosphere, the absorption flutings of the compounds of carbon should come in.

So much, then, for what we should expect, assuming the hypothesis to be true.

I now proceed to show how far these requirements are satisfied by the mass of new facts now at our disposal.

THE ACTUAL PHENOMENA RECORDED ON THE PHOTOGRAPHS.

Nebulæ.

The photographs of the spectrum of the Orion Nebula show lines at wave-lengths which approximate very closely to the lines of hydrogen, to flutings which appear in the spectra of compounds of carbon, to a fluting of magnesium at 5006, and to the longest flame lines of iron, calcium, and magnesium.

The chromospheric line designated D_3 has been recorded in the visual spectrum of the Orion Nebula by Dr. Copeland,[1] and the observation has since been confirmed by Mr. Taylor.[2]

The line which is always associated with D_3 in the spectrum of the chromosphere, viz. that at λ 4471 (Lorenzoni's *f*), is also shown in the photograph of the spectrum of the Orion Nebula.

The requirements of the hypothesis with regard to nebulæ are therefore met in every point so far considered by the new facts.

Dividing up the lines into the three groups of origins suggested, we have in the case of the Orion Nebula :—

(*a*) Spectrum of large interspace (= that of non-condensable gases driven out of the meteorites) = lines of hydrogen ; flutings of carbon.

(*b*) Spectrum of vapours produced by the large number of partial collisions = fluting of magnesium at λ 5006 ; low temperature lines of iron, calcium, and magnesium.[3]

(*c*) Spectrum of the vapours produced at a very high temperature by the relatively small number of end-on collisions. The solar chromosphere may be taken as indicating the spectrum associated with this very high temperature = chromospheric lines, $D_3 + \lambda\,4471$.[4]

Bright-Line Stars.

Prof. Pickering has shown that the Draper Memorial Photographs (copies of which he has very kindly forwarded me) prove that bright-line stars are intimately connected with the planetary nebulæ, the lines in the spectra being almost identical.

The main point of difference is that the chief nebular line near λ 5006 is not seen in the spectrum of bright-line stars, and this no doubt is due to the relative absence of feeble collisions as condensation goes on. The brightening of this line in the spectra of Nova Cygni and Nova Aurigæ, as the stars faded away, is sufficient evidence that it is associated with low temperature, and hence it is not surprising to find that it is absent from the spectra of the bright-line stars, which on this hypothesis are hotter than the nebulæ, since they are more condensed.

I have stated that we should expect the hydrogen lines to be

[1] *Monthly Notices*, vol. xlviii., p. 360.
[2] *Ibid.*, vol. xlix. p. 124.
[3] I have previously given evidence deduced from eye observations, indicating the presence of other low temperature flutings of manganese and magnesium.
[4] Since the lectures were delivered (and in this I summarised a paper I had previously sent in to the Royal Society), this part of the hypothesis has been enormously strengthened by the discovery of a new series of gases which the spectrum indicates are associated with the one giving the line D_3 which I discovered in 1868 and named helium. These new gases contain many lines in addition to D_3 and 4471, which appear both in the solar chromosphere and nebula of Orion and stars of increasing temperature.

fainter, and the carbon flutings, and the continuous spectrum to be brighter than in nebulæ.

(*a*) The hydrogen lines are decidedly less prominent. Indeed they were not recorded at all in the eye observations of γ Argûs (Arg.-Oeltz., 17681), of Wolf and Rayet's second and third stars in Cygnus,[1] but they are shown in Prof. Pickering's photographs.

(*b*) In my previous discussion of these bodies[2] I showed that there was evidence of a very considerable amount of carbon radiation in the visible region of the spectrum. Subsequent work and an examination of Prof. Pickering's photographs have strengthened this view.

(*c*) There can be no question as to the continuous spectrum being brighter in bright-line stars than in nebulæ.

Stars of Increasing Temperature.

(Stage 1.) We should expect the spectra to show—

(*a*) Absence of bright lines.

(*b*) The presence of dark metallic flutings.

(*c*) The presence of bright carbon flutings.

(*d*) Continuous absorption in the violet.

Many of the stellar photographs answer these requirements.

(*a*) They show no bright lines under normal conditions, but if the stars are variable, the disturbances which bring about the change of luminosity at maximum, produce bright lines in the spectrum as in the case of the spectrum of Mira Ceti photographed by Prof. Pickering.

(*b*) Dark flutings have been photographed in several spectra.

(*c*) The photographs appear to show the actual presence of carbon radiation ; further photographs are being obtained to carry on the inquiry.[3]

The stars of this class which have already been photographed at Kensington are well advanced in condensation, as indicated by the numerous dark lines, and all the flutings, both bright and dark, are confined to the region less refrangible than G. We should therefore not expect to get the more refrangible carbon flutings. It is among the least condensed stars that we should expect the bright carbon to be more manifest, and, indeed, in the spectrum of Mira Ceti photographed by Prof. Pickering, there is strong evidence of the presence of one of the more refrangible carbon bands commencing at λ 4215.

(*d*) The photographs fully demonstrate that there is a very considerable amount of continuous absorption in the ultra-violet or violet.

It must be added that the sequence of the spectra photographed resembles that deduced from eye observations, and the wonderful thing is that the observations of Dunér will bear the severe test which has thus been applied to them.

(Stage 2.) At this stage we should expect—

(*a*) Diminution in the amount of continuous absorption.

(*b*) Spectrum consisting of dark metallic lines, but possibly differing from the solar spectrum.

These conditions are fulfilled by the stars of which α Tauri and γ Cygni may be taken as types. The continuous absorption is least in the latter. These spectra show numerous metallic lines, but they do not exactly resemble the solar spectrum. The hydrogen lines are comparatively thin, while other lines have very different intensities as compared with lines in the solar spectrum.

In these stars we have to deal with the varying volatilities of the meteoretic constituents of the swarm, while in the case of stars which are cooling we have to deal with successive combinations rendered possible by the fall of temperature in a gaseous mass. Hence differences in the spectra are to be expected.

(Stage 3.) The phenomena which would be expected on the hypothesis, at this stage, are fully satisfied by such stars as α Cygni, β Orionis, ζ Orionis, ϵ Persei. In these stars there is

[1] *Roy. Soc. Proc.* vol. xliv., pp. 33-43.
[2] *Ibid.*
[3] Subsequent eye observations by myself and Mr. Fowler seemed to leave no doubt as to the presence of these bright carbon flutings (*Roy. Soc. Proc.*, vol. xlvii. p. 40). Dr. Copeland had previously made important observations of "Nova" Orionis with reference to this point (*Monthly Notices*, vol. xlvi. p. 112), and he identified one of the bright bands as "the great hydrocarbon band seen in the spectrum of every comet that has been examined under favourable circumstances." Referring to his observations of α Orionis, Mr. Maunder ("Greenwich Spect. Observations," 1889, p. 22) states that "the carbon band at 5164 was coincident (within the limits of observation with this dispersion) with the bright space towards the blue of Dunér's band 7."

no continuous absorption in the violet or ultra-violet, and the spectrum is one with simple line absorption, the iron lines quite disappearing after such a star as α Cygni is passed. The new lines which now make their appearance include the chromospheric line at λ 4471, and possibly a few others. It is important to note that the photographic region of the spectrum of the chromosphere has not yet been fully investigated, and hence a fair comparison with the spectra of these stars in the region F to K is not yet possible. M. Deslandres and Prof. Hale have photographed the chromospheric spectrum in the region more refrangible than H, but have not as yet published any account of the spectrum in the region now under discussion.

The Hottest Stars.

The conditions required by the hypothesis with regard to the stars at this stage are satisfied by such stars as ζ Cassiopeiæ and α Andromedæ.

In these stars we have—

(*a*) Broad lines of hydrogen, and

(*b*) Other absorption lines, chiefly of untraced origins, agreeing in position with some of the bright lines which appear in nebulæ.

It will be seen, then, that these considerations of the conditions of increasing temperature demanded by the hypothesis, have enabled us to determine that a long series of stellar spectra is in all probability a series in *ascending* order of temperature. All the phenomena we should expect, on the hypothesis, are met with among the photographs.

We have next to consider the phenomena connected with stars of decreasing temperature.

Stars of Decreasing Temperature.

(Stage 1.) With the failure of the supply of meteorites falling into the now vapourised mass, cooling will commence, and the longest lines in the spectra of the various chemical elements should make their appearance. This condition is met with and is well evidenced by the iron lines in the spectrum of Sirius.

(Stage 2.) The conditions at this stage of cooling are satisfied by δ Cassiopeiæ, β Cassiopeiæ, α Canis Minoris. In these stars we get, in addition to fairly broad lines of hydrogen, nearly all the lines which appear in the solar spectrum, and these, it is well known, agree in the main with the arc spectra of the various chemical elements.

(Stage 3.) Such stars as Capella and Arcturus represent the conditions which are required by the hypothesis at this stage of cooling. The metallic line absorption is again at a maximum, and we find the lines of the various chemical elements similar to those seen at Stage 2 of the ascending series, but with different intensities and with different amounts of continuous absorption at the violet end of the spectrum. This difference, so far as the known lines are concerned, will be due to a different percentage composition of the absorbing mass of vapour.

Continuous absorption in the violet recommences at this stage. There is undoubted evidence of carbon in the solar spectrum, and in the spectrum of Arcturus—the only star which has yet been investigated with special reference to this point.

Hence, it seems probable that "the indications of carbon will go on increasing in intensity slowly, until a stage is reached, when, owing to the reduction of temperature of the most effective absorbing layer, the chief absorption will be that of carbon."

It is evident that all such stars will be dim, and hence their spectra have not been met with in this preliminary survey of the photographic spectra of the brighter stars.

General Results of the Discussion.

The general result of the above discussion then, as far as it goes, is as follows:—Among the 171 stars already considered there are really two series of spectra, one representing the changes accompanying increase of temperature, while the other represents the effects of decreasing temperature. The fundamental requirement of the meteoritic hypothesis is, therefore, fully justified by the discussion of the photographs.

A very important point in connection with the two series of successive spectra is that one spectrum, such as that of α Andromedæ, possesses characteristics common to both, and we might, therefore, connect the two series together by this spectrum. In that case we should find, if we commence with the first spectrum in Series 1, say that of α Herculis, that the continuous absorption diminishes and that the breadth of the hydrogen lines regularly increases, until such a spectrum as that of α Andromedæ is reached. Then the condition would be reversed, the breadth of the hydrogen lines diminishing and the continuous absorption in the ultra-violet increasing in extent until such a star as Arcturus is reached.

It may be stated finally that the sequence now determined from the photographs follows exactly the same order as the groups originally suggested by the hypothesis, from a discussion of the eye observations. That is, it is not necessary to interchange any of the groups in order to obtain agreement with the photographic results.

J. NORMAN LOCKYER.

SCIENCE IN THE MAGAZINES.

PROFS. WEISMANN, Haeckel, and Karl Pearson will probably have something to say in reply to a paper which Dr. St. George Mivart contributes to the *Fortnightly*. The paper deals with what is described as "Denominational Science," in which dogma takes the place of facts, and persuasions are given out as if they were demonstrated truths. Dr. Weismann comes under Dr. St. George Mivart's displeasure in this regard; and a noteworthy characteristic of his is said to be "the confidence with which he propounds hypotheses which are either purely imaginary, or are only supported by an infinitesimal basis of fact, and the readiness with which he comes forward with a fresh gratuitous hypothesis, to replace others which have been refuted by newly-discovered truths." Prof. Haeckel is taken to task for the opinions expressed in his book on "Monism," lately translated into English. The bearing of Dr. St. George Mivart towards the book is indicated by the remark which opens the attack upon some of the points in it. We read: "It is difficult to say whether this small volume is more remarkable for the self-conceit and empty dogmatism, or for the ignorance it displays—ignorance concerning the most fundamental questions of which its treats." To assess these remarks at their proper value, it is necessary to read the article containing them, and the work to which they refer. Prof. Karl Pearson completes the trio upon whose views Dr. St. George Mivart outpours the vials of his wrath. His "Grammar of Science," and his remarks, in the *Fortnightly*, on Lord Salisbury's Oxford address, are given as evidence that "we have in England a denominational writer only second in self-confident dogmatism to Haeckel." All the members of the trio are held up as awful examples of "an unconscious slavery of the intellect to the mere faculty of the imaginal tion, and the consequent presentation of shallow and illogical imaginary phantasms as deep and far-reaching intellectual truths in the form of baseless dogmas of denominational science." Huxley and Karl Vogt are compared by Prof. Haeckel in the *Fortnightly*, the former being given a higher place than the latter, both as regards his philosophical reasonings, and because he showed a much deeper insight into the essence and import of scientific things. Two pages of the six, which form Prof. Haeckel's notice, are taken up with a denunciation of Prof. Virchow's antagonism to Darwinism, and the theory of descent, especially with reference to the most important deduction from the theory—the descent of man from the ape. Virchow's dissent in this matter is used as one of the sticks with which Mr. F. H. Hill belabours agnosticism, and Huxley's support of it, in the *National*, under the title, "Gaps in Agnostic Evolution."

Mr. Herbert Spencer continues his analysis of "Professional Institutions," in the *Contemporary*, the evolution of the biographer, historian, and man of letters being traced this month. "The primitive orator, poet, and musician," says Mr. Spencer, "was at the same time the primitive biographer, historian, and man of letters. The hero's deeds constituted the common subject-matter; and taking this or that form, the celebration of them became, now the oration, now the song, now the recited poem, now that personal history which constitutes a biography, now that larger history which associates the doings of one with the doings of many, and now that variously-developed comment on men's doings, and the course of things which constitute literature." Thus arose the rudiments of biography, history, and literature; and many facts illustrative of this early development are cited. Fiction developed out of biography and history, and gradually a class of story-tellers became differentiated. Indeed, for a time after fiction comes into existence, it is still classed and believed as biography. In our own times, we find

writers of history and biography and literature dividing into various classes, and finally there is the tendency of men of letters to unite into corporate bodies—an integration which has only become possible in recent years. In the same magazine, under the title "Heredity Once More," Dr. Weismann replies at length to an article contributed by Mr. Spencer to the magazine last October.

Mrs. Percy Frankland writes popularly on "Sunshine and Life," in *Longman's Magazine*, which also contains an account, by Mrs. A. Lang, of the Rev. John Mulso and his unpublished letters to Gilbert White, of Selborne, whose *alter ego* he was. Miss A. Lorrain Smith describes "Ants as Mushroom Growers" in an illustrated article in *Good Words*: her paper deals with the leaf-cutting and fungus-growing ants of Nicaragua. The *Sunday Magazine* has a second paper by "Eha," on Indian jungle life. *Knowledge* contains an account of Prof. Petrie's conclusions with regard to a "Newly-found Race in Egypt"; and papers on "Wind-Fertilised Flowers," by the Rev. A. S. Wilson; "Satellite Evolution," by Miss A. M. Clerke; "Photographs of Elliptical and Spiral Nebulæ" (with a plate), by Dr. J. Roberts; and "Blind Cave-Animals," by Mr. R. Lydekker. *Chambers's Journal* contains short popular articles on "Cordite and its Manufacture," and "The Prospects of our Descendants in regard to Stature," and a gossip on the Great Auk.

We have received, in addition to the periodicals named in the foregoing, *Scribner's Magazine*, and the *Humanitarian*.

ON THE ELECTROLYSIS OF GASES.[1]

IN the experiments described in this paper I have used the spectroscope to detect the decomposition of gases by the electric discharge and the movement of the ions in opposite directions along the discharge-tube.

The method consists in sending the electric discharge through a tube so arranged that the spectra close to the positive and negative electrodes can easily be compared, the presence or absence of certain ions at these electrodes can thus be ascertained. This method is capable of much wider application than the one I previously used in my experiments on the "Electrolysis of Steam" (*Proc. Roy. Soc.*, vol. lii. p. 90), the use of which is attended with very great difficulty for any substance other than steam. The earlier method has, however, the advantage of being a quantitative method—the present one is only qualitative.

In my former experiment with steam, when I worked at atmospheric pressure and varied the length of the spark, I found that when the spark-length exceeded a certain length, d_1, there was an excess of hydrogen at the negative electrode and of oxygen at the positive, equal in amount to the quantities of hydrogen and oxygen liberated from a water voltameter placed in series with the steam-tube. When the sparks were shorter than a certain length, d_2, the hydrogen appeared at the *positive*, the oxygen at the *negative* electrode, but the quantity of these gases was again equal to the quantities liberated in a water voltameter placed in series with the steam-tube.

When the spark-length was between d_1 and d_2 the effects were irregular, and there seemed to be no connection between the amounts of gases liberated in the steam-tubes and those liberated in the voltameter.

In the following experiments in which the sparks were of constant length and the pressure was altered, corresponding effects were observed. Within certain limits of pressure definite and perfectly regular evidence of the separation of the ions of the gas sparked through was obtained; and the electrode at which a given ion appeared could be reversed by altering the pressure; there was, however, a range of pressures in which the separation of the ions was either not well marked or was irregular in character.

I shall begin by describing a very simple method of showing the separation of the ions produced by the discharge of electricity through a compound gas such as hydrochloric acid gas, which is applicable when the discharges through the constituent gases of the compound are of distinct and different colours: this is eminently the case with the hydrochloric acid gas, as the discharge through hydrogen in a capillary tube is red, through chlorine green.

Take a capillary tube of very fine bore, the finer the better (the tube I used was thermometer tubing of the finest bore I could procure), and insert platinum wires for electrodes in two small bulbs blown on the ends of the tube; then fill the tube with HCl gas, allowing it to run through the tube for a considerable time so as to get rid of any extraneous gas, and exhaust the tube so that the gas in it is at a very low pressure. Then when the discharge from a large induction coil passes through the tube, the following phenomena are observed. When first the discharge passes through the tube the colour is uniform throughout and of a greenish-grey: after the discharge has been passing for a little time the end of the tube next the cathode gets distinctly red, whilst that next the anode gets green; this difference in the colour at the ends of the tube goes on increasing until the tube presents a most striking appearance, the part near the cathode being bright red, while that near the anode is a bright green. The difference in colour attains a maximum value, and if the discharge is allowed to run for several hours the contrast between the two ends disappears to a very great extent; the discharge throughout the whole of the tube being pinkish and apparently passing mainly through hydrogen. This is doubtless due to the diffusion through the tube of the hydrogen which in the earlier stages of the discharge had accumulated about the cathode; one advantage of using very narrow tubes is that with them this diffusion is slow. When the tube is in this condition the colour of the discharge sometimes changes suddenly, and for a second or two is green instead of pink, showing that though in the main the discharge passes through hydrogen, it occasionally leaves the hydrogen and passes through the chlorine. This transference of the discharge from one constituent to another of a mixture of gases is not infrequently observed when the gases are mixed in certain proportions.

Some of these capillary tubes showed after the discharge had been passing through them for some time a peculiar patchy appearance, some portions of the tube being a much brighter red than the others, while other portions were green. In some tubes this occurred to such an extent that the discharge showed an irregularly striated appearance. This effect is due, I believe, to gases or moisture condensed on the walls of the capillary tube, and in some cases to irregularities in the chemical composition of the glass. I found that it did not occur if the tube before being used was heated for some time along its whole length to as high a temperature as it would stand without collapsing; this heating would tend to cleanse the walls of the tube. That differences in the quality of the gas also conspire to produce these patches is shown, I think, by the following phenomenon. A capillary tube of fine bore containing mercury vapour and a little water vapour developed a well-marked red patch; the tube was then heated for some inches in the neighbourhood of the patch. In general heating the tube makes the discharge yellow from the sodium vapour given off from the glass; in this case, however, the whole of the heated portion, with the exception of the patch, turned yellow; the patch itself withstood the heating and continued to show the bright colour characteristic of hydrogen.

Electrolytic Transport of one Gas through another.—A tube of the shape shown in Fig. 1 was made of the finest bore thermometer-tubing; the extremities, C and D, of the tube in which

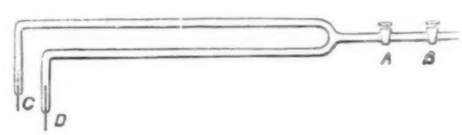

FIG. 1.

the electrodes were fused were bent down so as to be parallel to each other, and so near together that a slight motion of the tube suffices to bring either of the extremities in front of the slit of the spectroscope. The tube was mounted on a board moved by a lever; by moving this the observer at the spectroscope could readily bring the spectrum of either the positive or negative electrode into the field of view. A side tube, A B, was fused to the middle of the main tube and was provided with two taps; in the space between these taps a small quantity of any gas which it was desired to introduce into the main tube could be imprisoned, and could, by opening the tap A, be introduced into the discharge tube. The experiment consists in filling the main tube with a gas at a low pressure, observing the spectra at the

[1] Paper read at the Royal Society, by J. J. Thomson, M.A., F.R.S., Cavendish Professor of Experimental Physics, Cambridge.

two electrodes, then introducing by the side tube a very small quantity of gas into the main tube, and again observing the spectra at the two electrodes.

A tube was filled with hydrogen and showed no trace of the chlorine spectra; a very small quantity of chlorine was then let in through the side tube (in performing this experiment it is necessary to be careful that only a very small quantity of chlorine is introduced). After the discharge had been running through the tube for a short time, the chlorine spectrum was found to be bright at the positive electrode, though no trace of it could be detected at the negative. When the discharge was kept on for some time, the chlorine spectrum, though still visible at the positive electrode, got fainter; it did not appear at all at the negative. If a considerable quantity of chlorine was introduced through the side tube, the chlorine spectrum was visible at both electrodes, though it was brighter at the positive than at the negative.

When the induction coil was reversed, so that what was before the positive electrode became the negative, the first effect observed was that the chlorine spectrum flashed out with great brilliancy at the old positive electrode, and was much brighter than at any previous period. This, however, only lasted for a second or two; the chlorine spectrum rapidly faded away and for a time was not visible at either electrode. Soon, however, the chlorine spectrum appeared at the new positive electrode, having thus been transferred from one end of the tube to the other.

On again reversing the coil the same phenomenon was repeated. There is apparently no limit to the number of times this effect may be obtained; at any rate, I have driven the chlorine from one end of a tube to the other 14 times in succession by reversing the coil. The chlorine is always driven to the positive electrode, showing that the chlorine ion carries a charge of negative electricity. The same effect was obtained when a little vapour of bromine was introduced into the tube instead of chlorine. When, however, the capillary tube was filled with chlorine instead of hydrogen, and a little vapour of bromine let into the tube, the bromine went to the *negative electrode* instead of to the positive, as it did when introduced into the hydrogen tube. These experiments suggest that the two gases in the tube combine, and that the compound gas so formed is split up into ions which travel along the tube; that bromine when in combination with hydrogen is the negative ion, and therefore travels to the positive electrode; when, however, it is in combination with chlorine the bromine is the positive ion and travels to the negative electrode.

Another experiment tried was to let a little vapour of sodium into the middle of a capillary tube filled with air at a low pressure. To prevent the sodium vapour condensing on the walls of the tube, the whole tube was placed on a sand bath and the temperature raised so high that no condensation took place. After the discharge had run through the tube for about two hours the sand was removed from the tube, and the movement of the sodium vapour to the *negative* electrode was very apparent even without using a spectroscope, as there was a great patch of yellow light near the negative electrode and none in any other part of the tube.

Another experiment was to introduce a small quantity of hydrogen into a tube filled with air at a low pressure: the hydrogen made its way to the *negative* electrode. This experiment is a somewhat troublesome one, as it is exceedingly difficult to get these very fine capillary tubes so dry that the spectrum of the discharge does not show the hydrogen lines even before the hydrogen is introduced into the middle of the tube; indeed, I never succeeded in getting rid of the hydrogen lines at the very lowest pressures. By heating the tube and allowing dry air to run through it for a long time, however, I got the tube so dry that it did not show the hydrogen lines at a pressure quite low enough to allow the discharge to pass freely through it. When the tube was in this state and hydrogen was let into the middle of the tube, the hydrogen spectrum appeared at the negative electrode, but not at the positive.

The appearance of hydrogen at the negative electrode when mixed in a discharge tube with other gases has been described by Mr. Baly in a very interesting paper in the *Philosophical Magazine*, vol. xxxv. p. 200.

The preceding experiments suggest, I think, that this separation of two gases, A and B, by the discharge is due to the decomposition by the discharge of a chemical compound formed of A and B, in which the A atoms have a charge of electricity of one sign, the B atoms a charge of electricity of the opposite sign; these charged atoms under the influence of the electromotive force in the tube travel in opposite directions. Further, it follows from the experiment with the bromine vapour in an atmosphere of chlorine that the sign of the electrical charge on an atom of the same substance is not invariable, but depends on the substance with which this atom is in combination. We shall find numerous other instances of this change in the sign of the charge on an atom in experiments described in a later part of this paper.

Polarisation of the Electrodes.—This in the electrolysis of liquids is due to the accumulation at the electrodes of ions which have ceased to act as carriers of electricity. We have, I think, distinct evidence of a similar accumulation in the electrolysis of gases. For, as has been already described, after the discharge has been running for some time in one direction, giving the spectrum of some gas at one of the terminals, the spectrum of the gas at that terminal is momentarily brightened to a very great extent by suddenly reversing the direction of the discharge. After the current has been flowing for some time in one direction through, say, Cl in an atmosphere of H, the spectrum of the chlorine, though still visible at the positive electrode, gets faint, the chlorine apparently to a great extent ceasing to carry the discharge; when, however, the current is reversed, the atoms of chlorine can move freely, as they are not obstructed by the electrode, so that immediately after the reversal of the current there is probably more of the discharge carried by the chlorine than at any other time, and the chlorine spectrum is consequently brightest.

Discharge through a Compound Gas.—The separation of the ions by the discharge can be readily observed in a tube of the kind shown in Fig. 2.

It differs from an ordinary discharge tube merely in having a flat metal plate, A B, fastened across the tube. When the discharge passes through the tube, one side of the plate acts as a

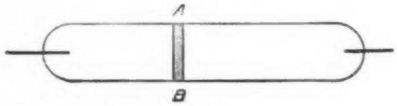

FIG. 2.

positive, the other as a negative, electrode. The tube is mounted on a stand, which the observer at the spectroscope can move by means of a lever so as to bring one side or other of the plate opposite the slit of the spectroscope; a very slight movement of the lever is sufficient to do this, so that the spectra at the two sides of the plate can readily be compared. I found that the results were more satisfactory when the current was kept flowing through the tube in one direction and the tube moved so as to bring the spectra at the two electrodes into the field of view than when the tube was kept fixed in one position and the current reversed. The latter method, however, suffices to show the separation of the ions in many cases, and it has the advantage of not requiring a plate across the tube; all that is necessary is to use for one of the terminals a disc whose plane is parallel to the slit of the spectroscope.

If the plate A B is thin, it is necessary to fuse it into the glass tube all the way round; otherwise, when the pressure is low, the discharge, instead of crossing the plate, goes through any little crevices there may be between the plate and the tube. The easiest way of making the tube is to use a plate about 0·5 cm. thick, cut from an aluminium cylinder which tightly fits the tube; with a plate of this thickness the narrow spaces between the tube and the plate are so long that the discharge goes through the plate rather than through the crevices.

The tube was filled with the gas to be observed and the spectra at the two sides of the plate compared. These spectra were in many cases found to differ in a very remarkable way; it was, however, only in exceptional cases that a line which was bright at one side of the plate was absolutely invisible on the other. The method used was to take two sets of lines, say A and B, as close together in the spectrum as possible, and compare the brightness of these sets of lines on the two sides of the plate; if it was found that the A lines were brighter on the positive side of the plate than on the negative, while on the other hand the B lines were brighter on the negative side of the plate than on the positive, then it was inferred that electrolytic

separation had occurred, and that the substance giving the A lines was in excess on the positive side of the plate, that giving the B lines on the negative. It is not safe to draw any conclusions from the variations in intensity of one line or one group of lines on the two sides of the plate, as the total quantity of light coming from the neighbourhood of the cathode often differs considerably from that coming from the anode. When, however, we get an increase in the brilliancy of one set of lines accompanied by a diminution in the brightness of another set, when we move across the plate we eliminate this source of error. The differences in the spectra at the two sides of the plate are most easily observed at pressures where there is not any very great difference between the luminosity of the cathode and the anode. As was mentioned at the beginning of the paper, there is a range of pressure within which the effects are irregular, and no decided differences are observed between the spectra at the two sides of the plate. It is desirable in these experiments to keep the tube on to the pipe as long as the experiment lasts, for the discharge always decomposes the compound gas, and unless the products of decomposition are continually pumped off and replaced by fresh supplies of the compound gas, the spectra of the discharge keep changing. With organic compounds this is especially necessary, as the character of the spectrum often changes entirely very shortly after the commencement of the discharge unless fresh gas is continually introduced.

In the following experiments the current was produced by a large induction coil with a mercury slow break.

When the tube was filled with hydrochloric acid gas at a low pressure, the separation of the hydrogen and chlorine was seen very distinctly, the hydrogen line being much brighter on the side of the plate which acted as the cathode (which we shall call the negative side of the plate) than on the positive side, while the chlorine, on the other hand, was brighter on the positive than on the negative side of the plate.

When the tube was filled with ammonia gas, the hydrogen lines were bright on the negative side of the plate, but were absent from the positive side, while on the positive side of the plate there was the positive pole spectrum of nitrogen, and on the negative side of the plate the negative pole spectrum of nitrogen and the hydrogen spectrum.

Sulphur Monochloride.—When the tube was filled with the vapour of this substance at a low pressure, the chlorine lines were brighter on the *negative* side of the plate than at the positive, while the sulphur lines were brighter at the positive side than at the negative. Thus the chlorine in this substance behaves in the opposite way to the chlorine in HCl; in the latter compound the chlorine iron has a charge of negative electricity, while in the sulphur monochloride it has a charge of positive electricity.

Influence of the Chemical Constitution of a Compound on the Sign of the Charge of Electricity on one of its Constituent Atoms.—In many organic compounds an atom of the electro-positive element hydrogen can be replaced by an atom of the electro-negative element chlorine without altering the type of the compound. Thus, for example, we can replace the four hydrogen atoms in CH_4 by chlorine atoms, getting successively the compound CH_3Cl, CH_2Cl_2, $CHCl_3$, and CCl_4. It seemed of interest to investigate what was the sign of the change of electricity on the chlorine atom in these compounds. The point is of some historical interest, as the possibility of substituting an electro-negative element in a compound for an electro-positive one was one of the chief objections assigned against the electro-chemical theory of Berzelius.

When the vapour of chloroform, $CHCl_3$, was placed in the tube, it was found that both the hydrogen and the chlorine lines were bright on the negative side of the plate, while they were absent from the positive side, and that any increase in the brightness of the hydrogen lines was accompanied by an increase in the brightness of those due to chlorine. The spectrum on the positive side of the plate was that called the carbonic oxide spectrum; when first the discharge passed through the tube, the spectrum on the positive side was the so-called candle spectrum, but this very rapidly changed to the carbonic oxide spectrum. The appearance of the hydrogen and chlorine spectra at the same side of the plate was also observed in methylene chloride and in ethylene chloride. Even when all the hydrogen in CH_4 was replaced by chlorine, as in carbon tetrachloride, CCl_4, the chlorine spectra still clung to the *negative* side of the plate. To test the point still further, I tried the analogous compound silicon tetrachloride, inserting a small jar in the circuit to brighten the spectrum. The chlorine spectrum was again brightest at the *negative* side of the plate, while the silicon spectrum was brightest at the positive. This is a very favourable case for the application of this method, as there are two silicon lines (wave-lengths 5058, 5043) quite close to two chlorine ones (wave-lengths 5102, 5078), so that their relative brightness can easily be compared. The experiment with the silicon tetrachloride is more conclusive than those with the carbon compounds, as with the latter the spectrum on the positive side of the plate is a band spectrum, and since the potential gradient when the discharge is passing is very much steeper on the negative side of the plate than on the positive, the effects observed might be supposed to be due to the circumstances on the negative side being better adapted for the production of line spectra than those on the positive. This explanation is not, however, applicable to the case of silicon tetrachloride, where the spectra on both sides of the plate are line spectra.

From these experiments it would appear that the chlorine atoms in the chlorine derivatives of methane are charged with electricity of the same sign as the hydrogen atoms they displace.

When we can determine the signs of the electrical charges carried by the atoms in a molecule of a compound, we can ascertain whether any given chemical reaction does or does not imply interchange between the electric charges on the atoms taking part in the reaction. Thus take the reaction

$$CH_4 + Cl_2 = CH_3Cl + HCl.$$

If we represent the sign of the charge of electricity carried by an atom by + or − placed below the symbol representing that atom, we may write the last reaction as

$$\underset{-}{C}\underset{+}{H}_4 + \underset{+}{Cl}\underset{-}{Cl} = \underset{-}{C}\underset{+}{H}_3\underset{+}{Cl} + \underset{+}{H}\underset{-}{Cl},$$

so that this reaction could be produced by a rearrangement of the atoms without any alterations of their electrical charges.

If, however, we take the reaction—

$$\underset{+}{H}\underset{-}{H} + \underset{+}{Cl}\underset{-}{Cl} = 2\underset{+}{H}\underset{-}{Cl},$$

we see that in addition to a rearrangement of the atoms there must in this case be an interchange of electric charges between the atoms; for before combination half the hydrogen atoms had a negative charge, and half the chlorine atoms a positive one, whereas after combination no hydrogen atom has a negative charge, and no chlorine atom a positive one. We may thus distinguish between two classes of chemical reactions, (1) those which do not necessarily require any interchange of the electrical charges carried by the atom, and (2) those which do. It might, perhaps, repay investigation to see whether the occurrence of chemical change is affected by the presence of a third substance in the same way in these classes of chemical combination.

Another point to be considered is the effect of this difference between the chemical actions on the amount of heat developed during chemical combination. When hydrogen and chlorine combine the heat produced may be regarded as the joint effect of three processes:—

(1) The splitting up of the molecules $(\underset{+}{H}\ \underset{-}{H})$ and $(\underset{+}{Cl}\ \underset{-}{Cl})$ into the atoms $\underset{+}{H}, \underset{-}{H}, \underset{+}{Cl}, \underset{-}{Cl}$.

(2) A transference of electricity by which the negative charge on one atom of hydrogen is replaced by an equal positive charge, while the positive charge on an atom of chlorine is replaced by an equal negative charge.

(3) The combination of the positively electrified hydrogen atoms with the negatively electrified chlorine ones to form hydrochloric acid.

In that class of chemical action where the atoms retain their charge (2) is absent, so that if the change in energy occurring in the process (2) were considerable compared with the changes occurring in processes (1) and (3), the thermal effects of the two types of chemical combination ought to differ considerably. If the changes in energy occurring in the process (2) had a great preponderance over those occurring in (1) and (3), the thermal effects produced by the combination of two elements ought to follow very simple laws. For if 2 {H} is the excess of the energy of an atom of hydrogen charged with the negative electron over the energy of the atom charged with the positive electron, 2 {Cl} the excess of the energy of an atom of chlorine charged with the positive electron over the energy of the atom charged with the negative electron, then if we could neglect the energy changes in (1) and (3) compared with those in (2), the

mechanical equivalent of the heat developed when a molecule of hydrogen combines with one of chlorine to form two molecules of hydrochloric acid would be equal to $2\{H\}+2\{Cl\}$. Thus we see that if the energy changes in (2) preponderated largely over those in (1) and (3), the heat produced when an element A combined with another element B to form the compound AB, could be expressed as the sum of two numbers $\{A\}$ and $\{B\}$, where $\{A\}$ depends solely on the element A, $\{B\}$ solely on the element B. In some cases of chemical combination between dilute solutions there seems evidence that the heat produced can be expressed in this way (see Lothar Meyer, "The Evolution of the Doctrine of Affinity," *Phil Mag.*, vol. xxiii. p. 504), but when we attempt to apply the same law to combination between gases, it seems utterly to break down; indicating that in such cases the greater part of the changes in energy occur in the splitting up of one set of molecules and the subsequent formation of others. This view seems to be supported by the phenomena attending the discharge of electricity through rarefied gases, for the smallest difference of potential which can send a discharge through an electrified gas (which we have reason to believe involves the splitting up of molecules into atoms), is very many times the electromotive force required to liberate the ions from an electrolyte, though the latter progress requires changes in the electrical charges on the ions. These reasons seem to indicate that we can hardly expect to get any clear indication of the charges carried by the atoms in gaseous compounds from the study of the thermal changes which occur when gases enter into chemical combination.

Vapours of Organic Compounds.—These show very interesting differences between the spectra on the two sides of the plate when the discharge passes through them. Thus when the discharge first passes through the vapour of ethyl alcohol, C_2H_6O, the spectrum on the positive side of the plate is the candle spectrum, that on the negative side the carbonic oxide spectrum. For some little time after the discharge commenced I could not detect any hydrogen lines on either side of the plate; after a time, however, they appeared on the negative side but not on the positive. If the discharge was kept running for some time without letting a fresh supply of alcohol into the tube the "candle spectrum" on the positive side of the plate was replaced by the CO spectrum, which now occurred on both sides of the plate accompanied on the negative side by the hydrogen spectrum. This is the appearance presented by all the compounds of carbon, oxygen, and hydrogen which I examined, when the spark had been passing through them for a considerable time, and it is what would occur if the vapour were decomposed by the spark into carbonic acid, water, and hydrogen.

The appearance of the candle spectrum on the positive side of the plate with the CO on the negative was observed in many other cases. Thus on sparking through a tube filled with CO, I could not detect any difference between the spectra on the two sides of the plate, but when a little hydrogen was let into the tube the "candle spectrum" appeared on the positive side of the plate, the carbonic oxide spectrum on the negative. The same effect was observed in a tube filled with cyanogen mixed with a little hydrogen. When the tube was filled with the vapour of methyl alcohol, CH_3OH, the candle spectrum was on the positive side of the plate, the carbonic oxide and hydrogen spectra on the negative; with this vapour, unlike that of ethyl alcohol, I could not detect any stage when the hydrogen spectrum was absent.

The first explanation which occurs to one of this phenomenon is that it is owing to the potential gradient at the negative side of the plate being steeper than that on the positive, so that we may imagine we have a fierce spark on the negative side, a mild one on the positive, and that the fierce spark gives the CO spectrum, the mild one the candle spectrum. There are, however, some phenomena which seem inconsistent with this explanation: in the first place, if the current is reversed after flowing in one direction, traces of the former spectra linger for some time at the sides of the plates, and, secondly, if the difference is due to the greater decomposition at the negative side of the plate, how is it that in the case of the vapour of ethyl alcohol the hydrogen spectrum is not seen, at the commencement of the discharge, on the negative side of the plate? it only appears after the discharge has passed through for some time, when hydrogen has probably been set free by the decomposition of the vapour by the discharge. If the absence of the candle spectrum from the negative side of the plate is due to the spark being so intense that the hydro-carbon which is supposed to be the origin of this spectrum cannot exist, then we ought to see the spectra of the substances which result from the decomposition of the hydro-carbon, *i.e.* we ought to see the hydrogen spectrum at the negative electrode. The view which seems most in accordance with the results of observations on the discharge through these vapours is that the "candle spectrum" is the spectrum of carbon when the atom is charged with negative electricity, or of some compound of carbon in which its atom is negatively charged, while the "carbonic oxide" spectrum is the spectrum of carbon when the atom is charged with positive electricity, or of some compound in which the carbon atom is positively charged.

Discharge through an Elementary Gas.—It has long been known that when the discharge passes through some elementary gases, the spectra at the two electrodes are different. This was first shown to be the case for nitrogen, then Dr. Schuster showed that the same thing occurred with oxygen, and recently Mr. Crookes has shown that it is also true in the case of argon. I have observed a very striking change in the relative brilliancy of the red and green hydrogen lines at the two electrodes. When the tube with the plate across it was filled with hydrogen at a low pressure, then on the positive side of the plate the red line tends to be brighter than the green, while on the negative side the green line tends to be brighter than the red; in some tubes this was so marked that on the positive side of the plate the red line was bright, and the green invisible, while on the negative side of the plate the green line was bright, and the red invisible. The spectroscope I was using weakened the red rays much more than the green, so that I cannot be sure that the red rays were really altogether obliterated on the negative side of the plate; the above experiment is, however, sufficient to show that on the positive side of the plate the red rays are more easily excited than the green, while on the negative side the green line is more easily excited than the red. On the negative side of the plate we have an excess of positively charged hydrogen atoms, while on the positive side of the plate there is an excess of negatively charged hydrogen atoms, and I am inclined to attribute the difference in the spectra partly at any rate to the difference in properties between a positively and a negatively charged hydrogen atom. The reason I do not attribute it wholly to the difference in the potential gradient on the two sides of the plate is that the effect is not reversed immediately, but only gradually on reversing the coil, the former spectra clinging for some time to the sides of the plate.

Chlorine.—I have made a great many experiments to see if there is any difference between the spectra given by chlorine on the two sides of the plate, but with negative results. Chlorine seems a gas in which we might expect to find this effect, for as Dr. Schuster, in his Report on Spectrum Analysis, says, the behaviour of its spectrum indicates that we have several spectra superposed. I have not, however, been able to affect a separation of its spectra, the differences I observed between the spectra on the two sides of the plate were irregular, and due, I think, to impurities producing effects like those observed when the discharge passes through a compound gas. However, as has been mentioned before, there is even in the case of gases where distinct evidence of separation can be obtained, a region of pressure within which the effects are irregular, and I ascribe my failure to observe separation in the case of chlorine to my having failed to get the relation between the intensity of the discharge and the pressure so adjusted as to get outside this irregular region. The cases, however, in which distinct differences between the spectra of a single gas occur at the two electrodes, seem to indicate that the spectrum given by an element is influenced by the sign of the electrical charge carried by its atoms.

I have made some experiments to determine whether there was any separation produced in a mixture of equal volumes of hydrogen and chlorine kept in the dark, when a considerable difference of potential though not sufficient to produce discharge was maintained between the two electrodes. The parts of the tube adjacent to the two electrodes could be shut off from each other by a tap, and the amount of chlorine in the two sides was determined by absorbing it by caustic potash. The mixture was at atmospheric pressure, and the electrodes were maintained at a potential difference of about 1200 volts by connecting them to a large battery of small storage cells. The potential difference between the terminals was maintained for about sixteen hours on three separate occasions, but on analysing the vessels surrounding the two electrodes, the amount of chlorine in the vessel adjacent

to the negative electrode did not differ from that in the vessel adjacent to the positive electrode by more than 1 per cent., and this could be accounted for by errors of experiments, as test experiments, in which the mixture had not been exposed to the electric field, gave differences comparable with these. We should conclude from the preceding experiments that the molecules of a gas are not acted on by any appreciable translational force tending to move them from one place to another, when they are near to a body charged with electricity. To test this point further, two large terminals were placed in bulbs which were connected by a horizontal capillary tube, in which a drop of sulphuric acid was placed; a difference in the pressure of the gas would cause the sulphuric acid to move, and the arrangement acts as a very delicate pressure gauge. The bulbs and tube were filled with chlorine at atmospheric pressure. The terminals were then connected to the electrodes of a battery giving a potential difference of 1200 volts, but not the slightest movement of the drop of acid could be detected.

I wish to acknowledge the help I have received in making the preceding experiment from my assistant, Mr. E. Everett.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

IN the ninth session of Edinburgh Summer Meeting, which was opened by Lord Reay on August 5, and has just concluded, natural science was represented by Elisée Reclus ("On the Evolution of Cities"), Dr. W. W. J. Nicol ("On Every-day Chemistry"), Mr. J. G. Goodchild ("On the Geology of Edinburgh"), Mr. A. J. Herbertson ("On the Geography of the District"), Dr. Louis Irvine ("On the Nervous System"), Mr. J. Arthur Thomson ("On the Biology of the Seasons"), Mr. R. Turnbull ("On Applied Botany"); Prof. Lloyd Morgan lectured "On Evolution Ethics," Prof. Haddon "On the Savage Mind," and Prof. Geddes "On Life and Thought."

MR. JOSEPH BISSETT, who was for two years lecturer at the Agricultural College, Aspatria, has been appointed Agricultural Lecturer to the County of Ayr.

MR. F. G. JONES goes to the Huddersfield Technical School as Lecturer in Physics, Applied Mechanics and Steam, and Mr. J. Brierley is to fill the post of Assistant Master in Chemistry and Physics at the same school.

THE Calendars for the Session 1895-96 of the University College, Bristol, and the Glasgow and West of Scotland Technical College have just been published, and may be obtained, respectively, of Arrowsmith, Bristol, and Anderson, Glasgow.

THE *Educational Times* understands that Mr. Arthur Milman will retire early in 1896 from the Registrarship of London University, under the Civil Service regulation as to age.

SCIENTIFIC SERIALS.

American Meteorological Journal, August.—The principal articles are:—Relation of clouds to rainfall, by H. Helm Clayton. A special study of cloud-forms before and after rain was made at the Blue Hill Observatory, and it was found that the most frequent succession of clouds preceding rain was cirrus, cirro-stratus, alto-stratus, and nimbus; the first which appeared in advance of the rain being usually cirrus. Rain was observed to fall from four classes of clouds: (1) a high cloud sheet (alto-nimbus); (2) a low, ragged cloud sheet (nimbus); (3) long, low rolls of cloud, giving light intermittent showers; and (4) a towering cloud of the cumulus type (cumulo-nimbus). Following rain, the most frequent clouds were strato-cumulus, in long, low rolls, while above there was most frequently cirrus or cirro-stratus. The result of the investigation showed that cloud-forms cannot, in general, be used in predicting rain for more than twenty-four hours in advance, but that, for a few hours in advance, the existence of certain clouds frequently furnish most trustworthy indications of coming rain.—The meteorograph for the Harvard Observatory on El Misti, Peru, by S. P. Fergusson. It has been found impossible to maintain observers at this elevated station (19,300 feet), and during the rainy season, which lasts three or four months, no ascent can be made. A meteorograph, on the principle of Richard's well-known instruments, has been constructed at the request of Prof. Pickering, which will work for four months, and will be installed on the summit of the mountain this summer. The record drum revolves once during three days, giving to the paper a speed of three inches in a day, and the paper used for the records is rolled upon a removable reel under the record drum. An illustration of the apparatus is given in the journal.

Bulletins de la Société d'Anthropologie de Paris, 1895, fas. 1.—Discussion of the *Pithecanthropus erectus* as the presumed precursor of man, by L. Manouvrier.—This paper contains a critical examination of the remains recently discovered by M. Dubois in Java, upon which an article by Prof. Cunningham has already appeared in NATURE.—The dolmen of Ethiau, by M. Lionel Bonnemère. After a careful examination of the marks upon the dolmen, the author has come to the conclusion that they are not due to atmospheric action but to the hand of man.—Lower terrace of Villefranche-sur-Saône, by M. G. de Mortillet. Many worked flints have been found associated with teeth of *Elephas primigenius* and *Rhinoceros tichorhinus*. At Chelles, the molars *E. antiquus* are common and characteristic, and the teeth of rhinoceros, which are very abundant, appear to belong to a small variety of *R. Merkii*.—The engraved stones of New Caledonia, by M. L. Bonnemère. The author exhibited, in the name of M. Glaumont, collector at Coron, a most interesting series of drawings made by him representing certain remarkable objects from the colony. Many large stones are covered with designs that were evidently executed before the European occupation of the island.

L'Anthropologie, 1895, No. 3.—General considerations on the Yellow Races, by Dr. E. T. Hamy. The opening lecture of the course of Anthropology at the Museum.—Infantilism, feminism, and antique hermaphrodites, by Henry Meige. Several cases in illustration of this paper have been drawn from the patients of Salpêtrière.—Studies in prehistoric ethnography, by Ed. Piette. Many archæologists have imagined that between the quaternary period and the modern era there was a long interval of desolation, during which the lands of Western Europe were devoid of inhabitants, and the record of human life was interrupted. They named it the *hiatus*. The author traces the history of the harpoon during this period, and shows that no such *hiatus* occurred.—Sculpture in Europe before the Greek-Roman Influences, by M. Salomon Reinach. In this section of M. Reinach's valuable monograph, the subject of gesture is treated, and numerous illustrations of bronze figures are given in illustration of the author's argument.

Bollettino della Società Sismologica Italiana, i., 1895, No. 4.—Vesuvian notices (1894), by G. Mercalli.—On the propagation in Italy of the Lubiana earthquake of April 14, 1895, by M. Baratta. A brief account, with a map showing the course of the isoseismal lines in Italy.—Notices of Italian earthquakes (April 1895). A valuable list of records, principally of the earthquake which forms the subject of the preceding paper.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 20.—"On the Refractive Index of Water at Temperatures between 0° and 10°." By Sir John Conroy, Bart., F.R.S.

In 1856, Jamin (*Comptes rendus*, vol. xliii. p. 1191) published an account of observations he made on the refractive index of water at temperatures between 30° and 0°. He used an interference method, and found that as the water cooled the index increased; similar results have been obtained by other observers, but although it appears to be proved that the refractive index of water increases with the decrease of temperature until the freezing point is reached, few determinations of the values of refractive indices of water near its point of maximum density have been published.

The method employed was the ordinary one, the determination of the angle of minimum deviation for a ray of definite wavelength passing through a hollow glass prism containing water at a known temperature.

The prism was filled with distilled water which had been recently boiled and allowed to cool under reduced pressure, and

was surrounded by a water-jacket, through which a stream of brine, cooled by a freezing mixture, could be passed.

The determinations were made exclusively with sodium light.

In the first column of the table the values of the refractive indices, relative to air, for each degree are given to five places; in the second the values as found by Walter, and in the third and fourth those for sodium light, given by Gladstone and Dale, and Rühlmann.

Refractive Indices of Water.

t.	C.	W.	t.	G and D.	t.	R.
0°	1·33397	1·33401	0°·0	1·33374	0°·0	1·33375
1	1·33397	1·33400	4·0	1·33367		1·33380
2	1·33396	1·33398	6·5	1·33356	1·5	1·33375
3	1·33394	1·33396	9·0	1·33342	4·0	1·33372
4	1·33392	1·33393	—	—	5·0	1·33371
5	1·33389	1·33390	—	—	5·8	1·33368
6	1·33385	1·33387	—	—	9·9	1·33355
7	1·33382	1·33383	—	—	10·0	1·33353
8	1·33378	1·33379	—	—	—	—
9	1·33375	1·33374	—	—	—	—

The values show that the refractive index of water, as was first announced by Jamin, increases continuously up to the freezing point, the rate of increase, however, seems to change about 4°, the temperature of maximum density, as was pointed out by Gladstone and Dale, and that no formula representing the variation of the refractive index of water with the temperature, as a function of the density only, can be a complete expression of the facts of the case.

PARIS.

Academy of Sciences, August 26.—M. Fizeau in the chair.—Truffles (*Terfâs*) from Cyprus (*Terfezia claveryi*), Smyrna, and La Calle (*Terfezia leonis*), by M. Ad. Chatin.—Observations of Swift's comet (August 20, 1895), made at Lyons Observatory by means of the coudé equatorial (0·32 m.), by M. G. Le Cadet. The remark is recorded that this comet appears as a very diffuse and feeble nebulosity almost equally spread in every direction. By oblique vision a nearly central feeble condensation can be distinguished.—Observations of the planet Phao (322), made at Marseilles Observatory by means of the 0·26 m. equatorial, by M. Borrelly.—On regular pencils and the *équitatères* of the *n*th order, by M. Paul Serret.—Heat of solution and of formation of sodium and potassium cyanurates, by M. Paul Lemoult. A detailed thermochemical study. The difficulty of forming the trimetallic salts is emphasised, and it is shown that the sodium and potassium series do not differ essentially. Water does not appear to decompose these salts.—On *apiculée* fermentation, and on the influence of aëration in elliptic fermentation at a high temperature, by MM. M. Rietsch and M. Herselm. Alcohol formed from *apiculée* yeasts requires more sugar for its production than that produced by the agency of elliptic yeasts. Cooling the must to just below 30° and aëration both favour the economical production of alcohol.—On aluminium utensils, by M. Balland. These utensils in ordinary camp use stand wear fairly well, and are not much attacked by foods during the short time they are in contact therewith. They should not be soldered or brought into contact with other metals. In the process of manufacture, treatment with soda should be avoided; the fine matt surface produced is more easily attacked than a polished surface.—On the rôle of the liver in the anticoagulant action of peptone, by MM. E. Gley and V. Pachon. The results of the authors' experiments appear to show that peptone does not itself exert any anticoagulant effect, but that it stimulates the production by the liver of some substance possessing anticoagulant properties.—The ellipsoidal stethoscope, by M. Ch. V. Zenger. A solid homogeneous wooden ellipsoid of revolution is cut by two planes perpendicular to the principal axis, and passing through the two foci. One flat end being placed as usual on the body surface, the ear perceives at the other focus the sounds of organ movements with remarkable intensity, and free from parasitic sounds formed in the air space of the ordinary instrument.—The electrodynamic system of the world, by M. Ch. V. Zenger.

GÖTTINGEN.

Royal Society of Sciences.—The *Nachrichten*, part 2 for 1895, contains the following memoirs of scientific interest:—

May 25.—O. Hölder: On groups whose order is free from squares.

June 15—A. Hurwitz: A fundamental theorem in the arithmetical theory of algebraic magnitudes. A. von Koenen: On the selection of points near Göttingen at which differences in the intensity of gravity may be expected in trial pendulum experiments. W. Schur: On the results of the first pendulum trials. W. Voigt: *In memoriam* F. E. Neumann.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Annual Report of the Department of Mines and Agriculture, N.S.W., for the Year 1894 (Sydney).—Diseases of Personality: Prof. Ribot, translated (Chicago, Open Court Publishing Company).—Analytical Key to the Natural Orders of Flowering Plants: F. Thonner (Sonnenschein).—Studies in the Evolutionary Psychology of Feeling: H. M. Stanley (Sonnenschein).—University College, Bristol, Calendar for the Session 1895-1896 (Bristol, Arrowsmith).—Origin of Plant Structures: Rev. G. Henslow (K. Paul).—Bourne's Handy Assurance Manual, 1895: W. Schooling (London).

PAMPHLETS.—Stenopaic or Pin-hole Photography: F. W. Mills and A. C. Ponton (Dawbarn).—University Correspondence College, Lon. Inter. Science and Prel. Sci. Guide, No. vii. (Red Lion Square).—Ditto Inter. Arts Guide, No. x. (Red Lion Square).

SERIALS.—Indian Museum Notes, Vol. 3, Nos. 4 and 5 (Calcutta).—Chambers's Journal, September (Chambers).—Contemporary Review, September (Isbister).—Good Words, September (Isbister).—Sunday Magazine, September (Isbister).—Humanitarian, September (Hutchinson).—National Review, September (Arnold).—Scribner's Magazine, September (Low).—Fortnightly Review, September (Chapman and Hall).—Clinical Sketches, No. 8, Vol. 2 (Smith, Elder).—Notes from the Leyden Museum, July (Leyden, Brill).—Journal of the Royal Microscopical Society, August (Williams and Norgate).—A Monograph of the Land and Freshwater Mollusca; J. W. Taylor, Part 2 (Leeds, Taylor).—Bulletin de l'Académie Royale des Sciences de Belgique, 65e Année, No. 7 (Bruxelles).—Zeitschrift für Physikalische Chemie, xvii. Band, 4 Heft (Leipzig).—Katalog der Bibliothek der K. Leopoldisch-Carolinischen Deutschen Akademie der Naturforscher, Sechste Liefg. (Halle).—Ditto Repertorium zu den Acta und Nova Acta der Akademie, Erster Band (Halle).—The Asclepiad, No. 43, Vol. xi. (Longmans).

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